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PHILOSOPHICAL TRANSACTIONS, GIVING SOME ACCOUNT OF THE Preſent Undertakings, Studies, *and* Labours, OF THE INGENIOUS, IN MANY Conſiderable Parts of the WORLD.

VOL. LX. For the Year 1770.

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in *Holbourn.*

M.DCC.LXXI.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume. And this information was thought the more neceſſary, not only as it has been the common opinion, that they were publiſhed by the authority, and under the direction, of the Society itſelf; but alſo, becauſe ſeveral authors, both at home and abroad, have in their writings called them the *Tranſactions of the Royal Society*. Whereas in truth the Society, as a body, never did intereſt themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſeable, that a Committee of their Members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance or ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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In the YEAR 1770;

WITH

The NAMES of the DONORS.

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Feb.

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Feb.

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PHILO-

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of a Journey to Mount* Etna, *in a Letter from the Honourable* William Hamilton, *His Majeſty's Envoy Extraordinary at* Naples, *to* Mathew Maty, M. D. *Sec.* R. S.

SIR, Naples, Oct. 17, 1769.

Read Jan. 18, 1770.

ENCOURAGED by the aſſurances you give me, in your laſt obliging letter of the 15th of June, that any new communication upon the ſubject of volcano's would be received with ſatisfaction by the Royal Society, I venture to ſend you the following account of my late obſervations upon Mount Etna, which you are at liberty to lay before our reſpectable Society, ſhould you think it worth its notice.

After having examined with much attention the operations of Mount Veſuvius, during the five years, that I have had the honour of reſiding as his majeſty's miniſter at this court, and after having carefully remarked the nature of the ſoil for fifteen miles round this capital, I am, in my own mind, well convinced, that the whole of it has been formed by exploſion. Many of the craters, from whence this matter has iſſued, are ſtill viſible; ſuch as the Salfaterra near Puzzole, the lake of Agnano, and near this lake a mountain compoſed of burnt matter, that has a very large crater ſurrounded with a wall to incloſe the wild boars, and deer, that are kept there for the diverſion of his Sicilian majeſty; it is called Aſtruni: the Monte Nuovo thrown up from the bottom of the lucrine lake in the year 1538, which has likewiſe its crater, and the lake of Averno. The iſlands of Niſida and Procida are entirely compoſed of burnt matter; the iſland of Iſchia is likewiſe compoſed of lava, pumice, and burnt matter; and there are in that iſland ſeveral viſible craters, from one of which, no longer ago than the year 1303, there iſſued a lava which ran into the ſea, and is ſtill in the ſame barren ſtate as the modern lavas of Veſuvius. After having, I ſay, been accuſtomed to theſe obſervations, I was well prepared to viſit the moſt ancient, and perhaps the moſt conſiderable volcano that exiſts; and I had the ſatisfaction of being thoroughly convinced there, of the formation of very conſiderable mountains by meer exploſion, having ſeen many ſuch on the ſides of Etna, as will be related hereafter.

On the 24th of June laſt, in the afternoon, I left Catania, a town ſituated at the foot of mount Etna, or as it is now called Mon Gibello, in company with lord Fortroſe and the canonico Recupero, an ingenious prieſt of Catania, who is the only perſon there, that is acquainted with the mountain: he is actually employed in writing its natural hiſtory, but I fear will not be able to compaſs ſo great and uſeful an undertaking, for want of proper encouragement.

We paſſed through the inferior diſtrict of the mountain called by its inhabitants La Regione Piemonteſe. It is well watered, exceedingly fertile and abounding with vines, and other fruit trees, where the lava, or, as it is called there, the Sciara, has had time to ſoften and gather ſoil ſufficient for vegetation, which I am convinced from many obſervations, unleſs aſſiſted by art, does not come to paſs for many ages, perhaps a thouſand years or more; the circuit of this lower region, forming the baſis of the great volcano, is upwards of one hundred Italian miles. The vines of Etna are kept low, quite the reverſe of thoſe on the borders of Veſuvius, and they produce a ſtronger wine, but not in ſo great abundance. The Piemonteſe diſtrict is covered with towns, villages, monaſteries, &c. and is well peopled, notwithſtanding the danger of ſuch a ſituation. Catania, ſo often deſtroyed by eruptions of Etna, and totally overthrown by an earthquake towards the end of the laſt century, has been re-built within theſe fifty years, and is now a conſiderable town, with at leaſt thirty-five thouſand inhabitants. I do not wonder at the ſeeming ſecurity, with which theſe parts are inhabited, having been ſo long witneſs to the ſame near mount

Veſuvius. The operations of nature are ſlow; great eruptions do not frequently happen, each flatters himſelf it will not happen in his time, or if it ſhould, that his tutelar ſaint will turn away the deſtructive lava from his grounds; then indeed the great fertility in the neighbourhoods of volcanos tempts people to inhabit them.

In about four hours of gradual aſcent we arrived at a little convent of benedictine monks, called St. Nicolo dell' Arena, about thirteen miles from Catania, and within a mile of the volcano from whence iſſued the laſt very great eruption in the year 1669, a circumſtantial account of which was ſent to our court by a lord Winchelſea, who happened to be then at Catania in his way home, from his embaſſy at Conſtantinople. His lordſhip's account is curious, and was printed in London ſoon after; I ſaw a copy of it at Palermo, in the library of the prince Torremuzzo *. We ſlept in the benedictines convent

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* It is intituled, A true and exact Relation of the late prodigious Earthquake and Eruption of Mount Ætna, or Monte Gibello; as it came in a letter written to his majeſty from Naples, by the right honourable the earl of Winchelſea, his majeſty's late ambaſſador at Conſtantinople, who in his return from thence, viſiting Catania in the iſland of Sicily, was an eyewitneſs of that dreadful ſpectacle; together with a more particular narrative of the ſame, as it is collected out of the ſeveral relations ſent from Catania; publiſhed by authority. Printed by T. Newcomb, in the Savoy, 1669, p. 38.

" I accepted, ſays the author, the invitation of the biſhop of
" Catania to ſtay a day with him, that ſo I might be the better
" able to inform your majeſty of that extraordinary fire, which
" comes from Mount Gibel, 15 miles diſtant from that city,
" which, for its horridneſs in the aſpect, for the vaſt quantity
" thereof (for it is 15 miles in length, and 7 in breadth), for its

the night of the 24th, and paſſed the next morning in obſerving the ravage made by the abovementioned terrible eruption, over the rich country of the Piemonteſe. The lava burſt out of a vineyard within

“ monſtrous devaſtation and quick progreſs, may be termed an
“ inundation of fire, a flood of fire, cinders, and burning ſtones,
“ burning with that rage as to advance into the ſea 600 yards,
“ and that to a mile in breadth, which I ſaw; and that which
“ did augment my admiration was, to ſee in the ſea this matter
“ like ragged rocks, burning in four fathom water, two fathom
“ higher than the ſea itſelf, ſome parts liquid, moving and throwing off,
“ not without great violence, the ſtones about it, which like a cruſt
“ of a vaſt bigneſs, and red hot, fell into the ſea every moment,
“ in ſome place or other, cauſing a great and horrible noiſe,
“ ſmoak, and hiſſing in the ſea; and thus more and more coming
“ after it, making a firm foundation in the ſea itſelf. I ſtayed
“ there from 9 a clock on Saturday morning, to ſeven next
“ morning” (this muſt have been towards the middle or latter
end of April); “ and this mountain of fire and ſtones with
“ cinders, had advanced into the ſea 20 yards at leaſt, in ſeveral
“ places; in the middle of this fire, which burnt in the ſea,
“ it hath formed like to a river, with its banks on each ſide very
“ ſteep and craggy, and in this channel moves the greateſt
“ quantity of this fire, which is the moſt liquid, with ſtones
“ of the ſame compoſition, and cinders all red hot, ſwimming
“ upon the fire of a great magnitude; from this river of fire
“ doth proceed under the great maſſe of the ſtones, which are
“ generally three fathoms high all over the country, where it
“ burns, and in other places much more. There are ſecret con-
“ duits or rivulets of this liquid matter, which communicates
“ fire and heat into all parts more or leſs, and melts the ſtones
“ and cinders by fits in thoſe places where it toucheth them,
“ over and over again; where it meets with rocks or houſes
“ of the ſame matter (as many are), they melt and go away
“ with the fire; where they find other compoſitions, they turn
“ them to lime or aſhes (as I am informed). The compoſition
“ of this fire, ſtones and cinders, are ſulphur, nitre, quick-
“ ſilver, ſal ammoniac, lead, iron, braſs, and all other metals.
“ It moves not regularly, nor conſtantly down hill; in

a mile of St. Nicolo', and by frequent exploſions of ſtones and aſhes, raiſed there a mountain, which, as near as I can judge, having aſcended it, is not leſs than half a mile perpendicular in height, and is certainly at leaſt three miles in circumference at its

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“ ſome places it hath made the valleys hills, and the hills
“ that are not high are now valleys. When it was night, I
“ went upon two towers, in divers places, and could plainly ſee
“ at ten miles diſtance, as we judged, the fire to begin to run
“ from the mountain in a direct line, the flame to aſcend as
“ high and as big as one of the highest & greateſt ſteeples in your ma-
“ jeſty's kingdoms, and to throw up great ſtones into the air; I
“ could diſcern the river of fire to deſcend the mountain of a
“ terrible fiery or red colour, and ſtones of a paler red to ſwim
“ thereon, and to be ſome as big as an ordinary table. We could
“ ſee this fire to move in ſeveral other places, and all the coun-
“ try covered with fire, aſcending with great flames, in many
“ places, ſmoaking like to a violent furnace of iron melted,
“ making a noiſe with the great pieces that fell, eſpecially thoſe
“ which fell into the ſea. A cavalier of Malta, who lives there,
“ and attended me, told me, that the river was as liquid where
“ it iſſues out of the mountain, as water, and came out like a
“ torrent with great violence, and is five or ſix fathom deep,
“ and as broad, and that no ſtones ſink therein. I aſſure your
“ majeſty, no pen can expreſs how terrible it is, nor can all
“ the art and induſtry of the world quench or divert that which
“ is burning in the country. In 40 days time it hath deſtroyed
“ the habitations of 27,000 perſons, made two hills of one,
“ 1000 paces high apiece, and one is four miles in compaſs;
“ of 20,000 perſons, which inhabited Catania, 3000 did only re-
“ main; all their goods are carried away, the cannons of braſs
“ are removed out of the caſtle, ſome great bells taken down,
“ the city-gates walled up next the fire, and preparations made
“ to abandon the city.

“ That night which I lay there, it rained aſhes all over the
“ city, and ten miles at ſea it troubled my eyes. This fire in
“ its progreſs met with a lake of four miles in compaſs, and it
“ was not only ſatisfied to fill it up, though it was four fathom
“ deep, but hath made of it a mountain.”

baſis. The lava that ran from it, and on which there are as yet no ſigns of vegetation, is fourteen miles in length, and in many parts ſix in breadth; it reached Catania, and deſtroyed part of its walls, buried an amphitheatre, an aqueduct, and many other monuments of its ancient grandeur, which, till then, had reſiſted the hand of time; and ran a conſiderable length into the ſea, ſo as to have once formed a beautiful and ſafe harbour; but it was ſoon after filled up by a freſh torrent of the ſame inflamed matter, a circumſtance the Catanians lament to this day, as they are without a port. There has been no ſuch eruption ſince, though there are ſigns of many, more terrible, that have preceded it.

For two or three miles round the mountain raiſed by this eruption, all is barren, and covered with aſhes; this ground, as well as the mountain itſelf, will in time certainly be as fertile as many other mountains in its neighbourhood, that have been likewiſe formed by exploſion. If the dates of theſe exploſions could be aſcertained, it would be very curious, and mark the progreſs of time with reſpect to the return of vegetation, as the mountains raiſed by them are in different ſtates; thoſe (which I imagine to be the moſt modern) are covered with aſhes only; others of an older date, with ſmall plants and herbs, and the moſt antient, with the largeſt timber trees I ever ſaw; but I believe the latter are ſo very ancient, as to be far out of the reach of hiſtory. At the foot of the mountain raiſed by the eruption of the year 1669, there is a hole, through which, by means of a rope, we deſcended into ſeveral ſubterraneous caverns, branching out and extending much farther and deeper than we choſe to venture, the cold there being exceſſive, and a violent wind frequently extinguiſhing ſome of our torches. Theſe caverns undoubtedly contained the lava that iſſued forth and extended, as I ſaid before, quite to Catania. There are many of theſe ſubterraneous cavities known, on other parts of Etna; ſuch as that, called by the peaſants, La Baracca Vecchia, another La Spelonca della Palomba (from the wild pidgeons building their neſts therein), and the cavern Thalia, mentioned by Boccaccio. Some of them are made uſe of as magazines for ſnow; the whole iſland of Sicily and Malta being ſupplied with this eſſential article (in a hot climate) from mount Etna; many more would be found, I dare ſay, if ſearched for, particularly near and under the craters from whence great lavas have iſſued, as the immenſe quantities of ſuch matter we ſee above ground muſt neceſſarily ſuppoſe very great hollows underneath.

After having paſſed the morning of the 25th in theſe obſervations, we proceeded through the ſecond, or middle region of Etna, called La Selvoſa, the woody, than which nothing can be more beautiful. On every ſide are mountains, or fragments of mountains, that have been thrown up by various ancient exploſions; there are ſome near as high as mount Veſuvius, one in particular, (as the canon our guide aſſured me, having meaſured it) is little leſs than one mile in perpendicular heighth, and five in circumference at its baſis. They are all more or leſs covered, even within their craters, as well as the rich valleys between them, with the largeſt oak, cheſnut, and firr trees, I every ſaw any where; and indeed it is from hence chiefly, that his Sicilian majeſty's dock-yards are ſupplied with timber. As this part of Etna was famous for its timber in the time of the tyrants of Syracuſa, and as it requires the great length of time I have already mentioned before the matter is fit for vegetation, we may conceive the great age of this reſpectable volcano. The cheſnut-trees predominated in the parts through which we paſſed, and, though of a very great ſize, are not to be compared to ſome on another part of the Regione Selvoſa, called Carpinetto. I have been told by many, and particularly by our guide, who had meaſured the largeſt there, called La Caſtagna di Cento Cavalli, that it is upwards of twenty-eight Neapolitan canes in circumference. Now as a Neapolitan cane is two yards and half a quarter, Engliſh meaſure, you may judge, ſir, of the immenſe ſize of this famous tree. It is hollow from age, but there is another near it almoſt as large and ſound; as it would have required a journey of two days to have viſited this extraordinary tree, and the weather being already very hot, I did not ſee it. It is amazing to me that trees ſhould flouriſh in ſo ſhallow a ſoil, for they cannot penetrate deep without meeting with a rock of lava, and indeed great part of the roots of the large trees we paſſed by are above ground, and have acquired, by the impreſſion of the air, a bark like that of their branches. In this part of the mountain, are the fineſt horned cattle in Sicily; we remarked in general that the horns of the Sicilian cattle are near twice the ſize of any we had ever ſeen; the cattle themſelves are of the common ſize. We paſſed by the lava of the laſt eruption in the year 1766, 1766, which has deſtroyed above four miles ſquare of the beautiful wood abovementioned. The mountain raiſed by this eruption abounds with ſulphur and ſalts, exactly reſembling thoſe of Veſuvius, ſpecimens of which I ſent ſome time ago to the late lord Morton.

In about five hours from the time we had left the convent of St. Nicolo dell' Arena, we arrived at the borders of the third region, called La Netta, or Scoperta, clean or uncovered, where we found a very ſharp air indeed; ſo that in the ſame day, the four ſeaſons of the year were ſenſibly felt by us, on this mountain; exceſſive ſummer heats in the Piemonteſe, ſpring and autumn temperature in the middle, and extreme cold of winter in the upper region. I could perceive, as we approached the latter, a gradual decreaſe of vegetation, and from large timber trees we came to the ſmaller ſhrubs and plants of the northern climates; I obſerved quantities of juniper and tanzey; our guide told us, that later in the ſeaſon there are numberleſs curious plants here, and that in ſome parts there are rhubarb and ſaffron in plenty. In Carrera's hiſtory of Catania, there is a liſt of all the plants and herbs of Etna, in alphabetical order.

Night coming on, we here pitched a tent and made a good fire, which was very neceſſary, for without it, and very warm cloathing, we ſhould ſurely have periſhed with cold; and at one of the clock in the morning of the 26th, we purſued our journey towards the great crater. We paſſed over valleys of ſnow that never melts, except there is an eruption of lava from the upper crater, which ſcarcely ever happens; the great eruptions are uſually from the middle region, the inflamed matter finding (as I ſuppoſe) its paſſage through ſome weak part, long before it can riſe to the exceſſive height of the upper region, the great mouth on the ſummit only ſerving as a common chimney to the volcano. In many places the ſnow is covered with a bed of aſhes, thrown out of the crater, and the ſun melting it in ſome parts makes this ground treacherous; but as we had with us, beſides our guide, a peaſant well accuſtomed to theſe valleys, we arrived ſafe at the foot of the little mountain of aſhes that crowns Etna, about an hour before the riſing of the ſun. This mountain is ſituated in a gently inclining plain, of about nine miles in circumference; it is about a quarter of a mile perpendicular in height, very ſteep, but not quite ſo ſteep as Veſuvius; it has been thrown up within theſe twenty-five or thirty years, as many people at Catania have told me they remembered when there was only a large chaſm or crater, in the midſt of the abovementioned plain. Till now the aſcent had been ſo gradual (for the top of Etna is not leſs than thirty miles from Catania, from whence the aſcent begins) as not to have been the leaſt fatiguing; and if it had not been for the ſnow, we might have rode upon our mules to the very foot of the little mountain, higher than which the canon our guide had never been: but as I ſaw that this little mountain was compoſed in the ſame manner as the top of Veſuvius, which, notwithſtanding the ſmoak iſſuing from every pore, is ſolid and firm, I made no ſcruple of going up to the edge of the crater, and my companions followed. The ſteep aſcent, the keenneſs of the air, the vapours of the ſulphur, and the violence of the wind, which obliged us ſeveral times to throw ourſelves flat upon our faces to avoid being over-turned by it, made this latter part of our expedition rather inconvenient and diſagreeable. Our guide, by way of comfort, aſſured us that there was generally much more wind in the upper region at this time.

Soon after we had ſeated ourſelves on the higheſt point of Etna, the ſun aroſe and diſplayed a ſcene that indeed paſſes all deſcription. The horizon lighting up by degrees, we diſcovered the greateſt part of Calabria, and the ſea on the other ſide of it; the Phare of Meſſina, the Lipari Iſlands, Stromboli with its ſmoaking top, though at above ſeventy miles diſtance, ſeemed to be juſt under our feet; we ſaw the whole iſland of Sicily, its rivers, towns, harbours, &c. as if we had been looking on a map. The iſland of Malta is low ground, and there was a hazineſs in that part of the horizon, ſo that we could not diſcern it; our guide aſſured us he had ſeen it diſtinctly at other times, which I can believe, as in other parts of the horizon, that were not hazy, we ſaw to a much greater diſtance; beſides, we had a clear view of Etna's top from our ſhip as we were going into the mouth of the harbour of Malta ſome weeks before; in ſhort, as I have ſince meaſured on a good chart, we took in at one view a circle of above nine hundred Engliſh miles. The pyramidal ſhadow of the mountain reached acroſs the whole iſland and far into the ſea on the other ſide. I counted from hence forty-four little mountains (little I call them in compariſon of their mother Etna, though they would appear great any where elſe) in the middle region on the Catania ſide, and many others on the other ſide of the mountain, all of a conical form, and each having its crater; many with timber trees flouriſhing both within and without their craters. The points of thoſe mountains, that I imagine to be the moſt ancient, are blunted, and the craters of courſe more extenſive and leſs deep than thoſe of the mountains formed by exploſions of a later date, and which preſerve their pyramidal form entire. Some have been ſo far mouldered down by time as to have no other appearance of a crater than a ſort of dimple or hollow on their rounded tops, others with only half or a third part of their cone ſtanding; the parts that are wanting having mouldered down, or perhaps been detached from them by earthquakes, which are here very frequent. All however have been evidently raiſed by exploſion; and I believe, upon examination, many of the whimſical ſhapes of mountains in other parts of the world would prove to have been occaſioned by the ſame natural operations. I obſerved that theſe mountains were generally in lines or ridges; they have moſtly a fracture on one ſide, the ſame as in the little mountains raiſed by exploſion on the ſides of Veſuvius, of which there are eight or nine. This fracture is occaſioned by the lava's forcing its way out, which operation I have deſcribed in my account of the laſt eruption of Veſuvius. Whenever I ſhall meet with a mountain, in any part of the world, whoſe form is regularly conical, with a hollow crater on its top, and one ſide broken, I ſhall be apt to decide ſuch a mountain's having been formed by an eruption, as both on Etna and Veſuvius the mountains formed by

exploſion

explоſion are without exception according to this deſcription; but to return to my narrative.

After having feaſted our eyes with the glorious proſpect above-mentioned (for which, as Spartian tells us, the emperor Adrian was at the trouble of aſcending Etna), we looked into the great crater, which, as near as we could judge, is about two miles and a half in circumference; we did not think it ſafe to go round and meaſure it, as ſome parts ſeemed to be very tender ground. The inſide of the crater, which is incruſted with ſalts and ſulphurs like that of Veſuvius, is in the form of an inverted hollow cone, and its depth nearly anſwers to the height of the little mountain that crowns the great volcano. The ſmoak, iſſuing abundantly from the ſides and bottom, prevented our ſeeing quite down; but the wind clearing away the ſmoak from time to time, I ſaw this inverted cone contracted almoſt to a point; and, from repeated obſervations, I dare ſay, that in all volcanos, the depth of the craters will be found to correſpond nearly to the height of the conical mountains of cinders which uſually crown them: in ſhort, I look upon the cratersas a ſort of ſuſpended funnels, under which are vaſt caverns and abyſſes. The formation of ſuch conical mountains with their craters are eaſily accounted for, by the fall of the ſtones, cinders, and aſhes, emitted at the time of an eruption.

The ſmoak of Etna, though very ſulphureous, did not appear to me ſo fetid and diſagreeable as that of Veſuvius; but our guide told me that its quality varies, as I know that of Veſuvius does, according to the quality of the matter then in motion within. The air was ſo very pure and keen in the whole upper region of Etna, and particularly in the moſt elevated parts of it, that we had a difficulty in reſpiration, and that, independent of the ſulphureous vapour. I brought two barometers and a thermometer with me from Naples, intending to have left one with a perſon at the foot of the mountain, whilſt we made our obſervations with the other, at ſun-riſing, on the ſummit; but one barometer was unluckily ſpoilt at ſea, and I could find no one expert enough at Catania to repair it; what is extraordinary, I do not recollect having ſeen a barometer in any part of Sicily. At the foot of Etna, the 24th, when we made our firſt obſervation, the quickſilver ſtood at 27 degrees 4 lines, and the 26th, at the moſt elevated point of the volcano, it was at 18 degrees 10 lines. The thermometer, on the firſt obſervation at the foot of the mountain was at 84 degrees, and on the ſecond at the crater at 56. The weather had not changed in any reſpect, and was equally fine and clear, the 24th and 26th. We found it difficult to manage our barometer in the extreme cold and high wind on the top of Etna; but from the moſt exact obſervations we could make, in our circumſtances, the reſult was as abovementioned. The canon aſſured me, that the perpendicular height of mount Etna is ſomething more than three Italian miles, and I verily believe it is ſo.

After having paſſed at leaſt three hours on the crater, we deſcended and went to a riſing ground, about a mile diſtant from the upper mountain we had juſt left, and ſaw there ſome remains of the foundation of an ancient building; it is of brick, and ſeems to have been ornamented with white marble, many fragments of which are ſcattered about. It is called the Philoſopher's Tower, and is ſaid to have been inhabited by Empedocles. As the ancients uſed to ſacrifice to the celeſtial gods on the top of Etna, it may very well be the ruin of a temple that ſerved for that purpoſe. From hence we went a little further over the inclined plain abovementioned, and ſaw the evident marks of a dreadful torrent of hot water that came out of the great crater at the time of an eruption of lava in the year 1755, and upon which phænomenon the canonico Recupero, our guide, has publiſhed a diſſertation. Luckily this torrent did not take its courſe over the inhabited parts of the mountain, as a like accident on mount Veſuvius in 1631 ſwept away ſome towns and villages in its neighbourhood, with thouſands of their inhabitants. The common received opinion is, that theſe eruptions of water proceed from the volcanos having a communication with the ſea; but I rather believe them to proceed merely from depoſitions of rain water in ſome of the inward cavities of them. We likewiſe ſaw from hence the whole courſe of an ancient lava, the moſt conſiderable as to its extent of any known here; it ran into the ſea near Taormina, which is not leſs than thirty miles from the crater whence it iſſued, and is in many parts fifteen miles in breadth. As the lavas of Etna are very commonly fifteen and twenty miles in length, ſix or ſeven in breadth, and fifty feet or more in depth, you may judge, ſir, of the prodigious quantities of matter emitted in a great eruption of this mountain, and of the vaſt cavities there muſt neceſſarily be within its bowels. The moſt extenſive lavas of Veſuvius do not exceed ſeven miles in length; the operations of nature on the one mountain and the other are certainly the ſame; but on mount Etna, all are upon a great ſcale. As to the nature and quality of their lavas, they are much the ſame; but I think thoſe of Etna rather blacker, and in general more porous, than thoſe of Veſuvius. In the parts of Etna that we went over I ſaw no ſtratas of pumice ſtones, which are frequent near Veſuvius, and cover the ancient city of Pompeia; but our guide told us, that there are ſuch in other parts of the mountain. I ſaw ſome ſtratas of what is called here Tuffa, it is the ſame that covers Herculaneum, and that compoſes moſt of the high grounds about Naples; it is upon examination a mixture of ſmall pumice ſtones, aſhes, and fragments of lava, which is by time hardened into a ſort of ſtone. In ſhort I found, with reſpect to the matter erupted, nothing on mount Etna that Veſuvius does not produce, and there certainly is a much greater variety in the erupted matter and lavas of the latter, than of the former; both abound with pyrites and cryſtallizations, or rather vitrifications. The ſea ſhore at the foot of Etna, indeed, abounds with amber, of which there is none found at the foot of Veſuvius. At preſent there is a much greater quantity of ſulphur and ſalts on the top of Veſuvius than on that of Etna; but this circumſtance varies according to the degree of fermentation within, and our guide aſſured me he had ſeen greater quantities on Etna at other times. In our way back to Catania, the canon ſhewed me a little hill covered with vines, which belonged to the jeſuits, and, as is well atteſted, was undermined by the lava in the year 1669, and tranſported half a mile from the place where it ſtood, without having damaged the vines.

In great eruptions of Etna, the ſame ſort of lightening, as deſcribed in my account of the laſt eruption of Veſuvius, has been frequently ſeen to iſſue from the ſmoak of its great crater. The ancients took notice of the ſame phænomenon, for Seneca (lib. ii. Quæſt. Nat.) ſays,—"Ætna aliquando multo igne "abundavit, ingentem vim arenæ urentis effudit, "involutus eſt dies pulvere, populoſque ſubita nox "terruit, *illo tempore aiunt plurima fuiſſe tonitrua et* "*fulmina.*"

Till the year 252 of Chriſt, the chronological accounts of the eruptions of Etna are very imperfect; but as the veil of St. Agatha was in that year firſt oppoſed to check the violence of the torrents of lava, and has ever ſince been produced at the time of great eruptions, the miracles attributed to its influence having been carefully recorded by the prieſts, have at leaſt preſerved the dates of ſuch eruptions. The relicks of St. Januarius have rendered the ſame ſervice to the lovers of natural hiſtory, by recording the great eruptions of Veſuvius. I find, by the dates of the eruptions of Etna, that it is as irregular and uncertain in its operations as Veſuvius. The laſt eruption was in 1766.

On our return from Meſſina to Naples, we were becalmed three days in the midſt of the Lipari Iſlands, by which we had an opportunity of ſeeing that they have all been evidently formed by exploſion; one of them, called Vulcano, is in the ſame ſtate as the Solfaterra. Stromboli is a volcano, exiſting in all its force, and, in its form of courſe, is the moſt pyramidal of all the Lipari Iſlands; we ſaw it throw up red hot ſtones from its crater frequently, and ſome ſmall ſtreams of lava iſſued from its ſide, and ran into the ſea. This volcano differs from Etna and Veſuvius, by its continually emitting fire, and ſeldom any lava; notwithſtanding its continual exploſions, this iſland is inhabited, on one ſide, by about an hundred families.

Theſe, as well as I can recollect, are all the obſervations that I made with reſpect to volcanos, in my late curious tour of Sicily; and I ſhall be very happy ſhould the communication of them afford you, or any of our countrymen (lovers of natural hiſtory), ſatisfaction or entertainment.

I am, ſir,

with great regard and eſteem,

your moſt obedient

humble ſervant,

Wm. Hamilton.

II. *A Letter from* Philip Carteret *Eſquire, Captain of the* Swallow Sloop, *to* Mathew Maty, M. D. *Sec.* R. S. *on the Inhabitants of the Coaſt of* Patagonia.

On board the Swallow, in Port Famine,
Streights of Magellan, 11th January,
1767.

S I R,

Read Jan. 25, 1770.

THE Patagonians having made ſo much noiſe of late in Europe, and particularly in England, I imagine a more particular and certain account of them will not be diſagreeable to my good friend Doctor Maty.

In the morning of the 16th December, 1766, we were cloſe in with the entrance of the river Gallegoes; the country about which river, I have ſome reaſon to imagine, is the place of their common abode. I ſhall forbear to mention my reaſons for this ſuppoſition, as it would take too much room in this letter. From thence we ſailed along in ſight of the ſhore as far as cape Virgin Mary, which is the northermoſt promontory of the eaſtermoſt entrance of the ſtreights of Magellan. There ſeems to be but a ſhort diſtance over acroſs this kind of iſthmus to the river Gallegoes. As we kept ſailing along the ſhore, we ſaw ſome people riding on horſes over this part of the peninſula or neck of land, towards the place they ſaw we were making for with the ſhips. At the cloſe of day, as the wind was contrary, we anchored, within three or four miles of the above Cape. All the night long theſe people kept making fires on the beach, abreaſt of the ſhips, with a great hollowing noiſe for us to come on ſhore to them, which we did early the next morning with ſome armed boats; but I believe we had no occaſion for this (although eſſential and proper) precaution, for theſe people received us in a civil, friendly, and pretty regular manner. They all readily ſate down at ſome diſtance from us, at our deſire, by ſigns to them: and we then went amongſt them. Captain Wallis, of the Dolphin, gave them beads, ribbons, and ſome trifling cutlery, &c. at all which they ſeemed to be well pleaſed. They were between ſixty and ſeventy at this time, but their numbers kept increaſing, as ſome continually came down to the ſea ſhore; and before the next morning they were increaſed to ſeveral hundreds, men, women, and children. In the evening, having been obliged to anchor again, one of our boats, in which were ſeveral officers, went near the beach, abreaſt where the ſhips lay; thoſe people having followed us here, they endeavoured by all the friendly invitations they could make to entice our men to land; but as they had orders from captain Wallis to the contrary, they did not. When they ſaw that our people would not come on ſhore to them, they all drew off at a diſtance, leaving their children by the water-ſide. This I take to have been done to ſhew we had no occaſion to be apprehenſive of any danger and to expreſs their friendly intentions, and the confidence they had of ours; and probably likewiſe, from our having taken pretty much knowledge of their children, by dreſſing ſome of them with beads and ribbons, when we were on ſhore before. We meaſured the heights of many of theſe people; they were in general all from ſix feet, to ſix feet five inches, although there were ſome who came to ſix feet ſeven inches, but none above that. They are well proportioned, their features large and pretty regular, with pretty clear complections; and they would be much more ſo, if they did not paint, and expoſe themſelves to the inclemency of the weather, any more than we do in Europe. I am ſure there are many in Europe as dark coloured as they are; they have long black hair (but I think not ſo very black as the generality of the Indians), which many of them had tied up with a kind of woven ſtuff, of about the breadth of a garter, and ſeemed to have been made from ſome kind of wool. They ſeem to have but little hair on their bodies, and very ſmall breaſt, little beard, for I believe they pull it out by the roots, for many of them had little tufts left growing at each corner of the mouth, on the upper lips, and one in the hollow part of the chin, in the middle of the under lip; many had the hair of the eye-brows plucked off, and ſome had their faces painted. They are cloathed in ſkins, which they wear with the fur part next to the body; it hangs from their ſhoulders half-way down their legs, with a girdle tied about their waiſt, which makes their cloathing very decent, and they ſeem to be modeſt both in their actions and behaviour, at leaſt as much as we ſaw of it. They wear a kind of boots, which ſerves them likewiſe for ſhoes; but I know not if they wear any kind of drawers or breeches. The upper part of their garments being looſe about their ſhoulders, they did not ſeem cautious of hiding from ſight that part of their bodies; they have fine white teeth, but their hands and feet ſeemed to me rather ſmall, for ſuch ſtout, luſty people, by which I imagine they are not uſed to much hard work. The men and women are cloathed alike, ſo that it requires a little attention to diſtinguiſh the ſex. Altogether they are the fineſt ſet of men I ever ſaw any where before: there ſeems to be ſome ſubordination amongſt them; ſome had their cloathing painted on the outſide, in different ſquares, ſtrokes, and lines; theſe I obſerved ſeemed to have ſome authority over the reſt. When we went to re-imbark in our boats to return to the ſhips, moſt of them wanted to go off with us; we took a few of them in each boat, and carried them on board of each ſhip, by which the companies curioſity was ſatisfied as well as ours. We regaled them with ſuch things as we had; they eat ſalt beef, but ſeemed moſt fond of the ſhip's biſcuit, nor did they ſeem to like much the wine or ſtrong liquors, but drank water by tumblers full. We gave them pipes of tobacco, which they ſmoaked, and did not ſeem ſtrangers to it; they ſwallowed the ſmoak, as the Turks and many other nations do. They ſeemed very free and eaſy, no ways miſtruſtful, or afraid to truſt themſelves in any part of the ſhip with us; and they very willingly would have ſtayed longer with us, for we found ſome little difficulty to make them go back aſhore. When they found we wanted them to go away, they pointed to the ſun, ſhewing us its courſe with their hands, and by their ſigns intimated, that as the ſun was ſo high there was no occaſion, and that it would be time enough when it came to go down. They did not ſeem inclinable to thieve, or take any thing without its being given them, for if they took a fancy to any thing they ſaw, they made ſigns for it; and if we did not appear inclinable to give it them, they did not offer to take it. They ſeemed to be very intelligent and quick of apprehenſion, and pronounced very well and diſtinctly many Engliſh words after us. We often heard them repeat the word *Chovoa*, but what it meant we could not find out; they made uſe of the word *Capatana*, which I ſuppoſe is from the Spaniards, for captain. They had not any arms with them, ſo I cannot ſay what may be their common weapons; they all had a couple of round ſtones, like two balls, which are ſlung, one at each end of a cord, which is about one fathom and a half long; and with theſe two balls I believe they kill moſt of their game. The method of uſing them is, by keeping one of the balls in their hand, and ſwinging the other at the full end of the line, round over their heads, by which it acquires a greater velocity, and they throw it with a prodigious force at a great diſtance, and exactneſs, ſo as to ſtrike a very ſmall object. We ſaw one of them kill a ſeal from on horſeback in the ſurf of the ſea, on the beach; but in this action, he kept hold of one ball in his hand, while he hit the ſeal with the other; but I know not if they make uſe of theſe balls in fighting with men. Their horſes are of the Spaniſh breed, and ſeem to be of a good kind, about 14 to 15 hands high, of different and mixed colours; and from what I ſaw, I believe they ride them very hard, and do not uſe them very well. Theſe people have certainly trade and communication with the Spaniards, for one of them had a Spaniſh broad ſword, and he was the only perſon who was armed amongſt them; they had bridles, ſaddles, ſtirrups, and whips of ſkins, all of their own making; ſome had iron, and other metal bits to their bridles, and we ſaw ſome metal ſpurs. They had a dead oſtrich, the fleſh of which I ſaw ſome eat raw, but whether that be their common method of eating fleſh, I know not. I did not ſee any more of theſe fine people, although the ſlow progreſs we made by the contrary winds, for ſeveral days here about, gave us a fine opportunity of being better acquainted with them, and particularly as they kept on the ſea ſhore all the time to the number of three or four hundred. I was not a little chagrined, to find captain Wallis was averſe to it, and gave orders nobody ſhould go on ſhore to them; by this we loſt a very fine and favourable opportunity of knowing more of them, and of their country; the knowledge of which in all probability might be of ſervice to Great Britain. It was thought ſo formerly, when ſir John Narborough was ſent out by king Charles the ſecond, to endeavour to open a communication with theſe Indians, for I take them to be the very ſame nation, called by the Spaniards the Bravoes, who have often made them feel their courage and reſolution in the kingdom of Chili. They were the people, who defeated the great general Baldivia, and afterwards deſtroyed him by pouring melted gold in his mouth. It would have been very eaſy, ſince they were ſo inclinable to ſtay with us, to have taken one or two, and to have brought them as far as this place; we might have ſent them back by our ſtoreſhip, who came back this way, about a month after: during this time, by uſing of them well, we might have got their confidence and friendſhip, and have learnt ſome particulars of their country, which could not but have proved beneficial to our country. You may depend on the veracity of the above account, and that I am, with great ſincerity,

SIR,

Your moſt obedient and

moſt humble ſervant,

Ph. Carteret.

N. B. This is the copy of the letter I had ſent you from Port Famine, by the return of our ſtore-ſhip, which you told me the other day you never received, and muſt have been loſt.

April 20, 1769.

III. *A Letter on a* Camelopardalis *found about the* Cape of Good Hope, *from Capt.* Carteret *to* Mathew Maty, *M. D. Sec.* R. S.

On board of the Swallow, Deptford,
20th April, 1769.

SIR,

Read Jan. 25, 1770.

INCLOSED I have ſent you the drawing of a Camelopardalis, (TAB. I.) as it was taken off, from life, of one near the Cape of Good Hope. I ſhall not attempt here to give you any particular deſcription of this ſcarce and curious animal, as it is much better known to you than it can be to me; but from its ſcarcity, as I believe none have been ſeen in Europe ſince Julius Cæſar's time (when I think there were two of them at Rome), I imagine its drawing, and a more certain knowledge of its reality, will not be diſagreeable to you. As the exiſtence of this fine animal has been doubted by many, if you think it may afford any pleaſure to the curious, you will make what uſe of it you pleaſe.

The preſent governor of the Cape of Good Hope has ſent out parties of men on inland diſcoveries, ſome of which have been abſent from eighteen months to two years, in which traverſe they have diſcovered many curioſities, which it is to be hoped they will in convenient time communicate to the world. One of theſe parties croſſed many mountains and plains, in one of which they found two of theſe creatures, but they only caught the young one, of which the incloſed is the drawing, as it was taken off by them; they endeavoured to bring him alive to the Cape Town, but unfortunately it died. They took off his ſkin, which they brought as a confirmation of the truth, and it has been ſent to Holland *. Theſe particulars, as well as drawing, I got from Mr. Barrawke, firſt ſecretary to the Dutch company at that place, in the preſence of the governor.

I am,

SIR,

Your moſt humble,

moſt obedient ſervant,

Ph. Carteret.

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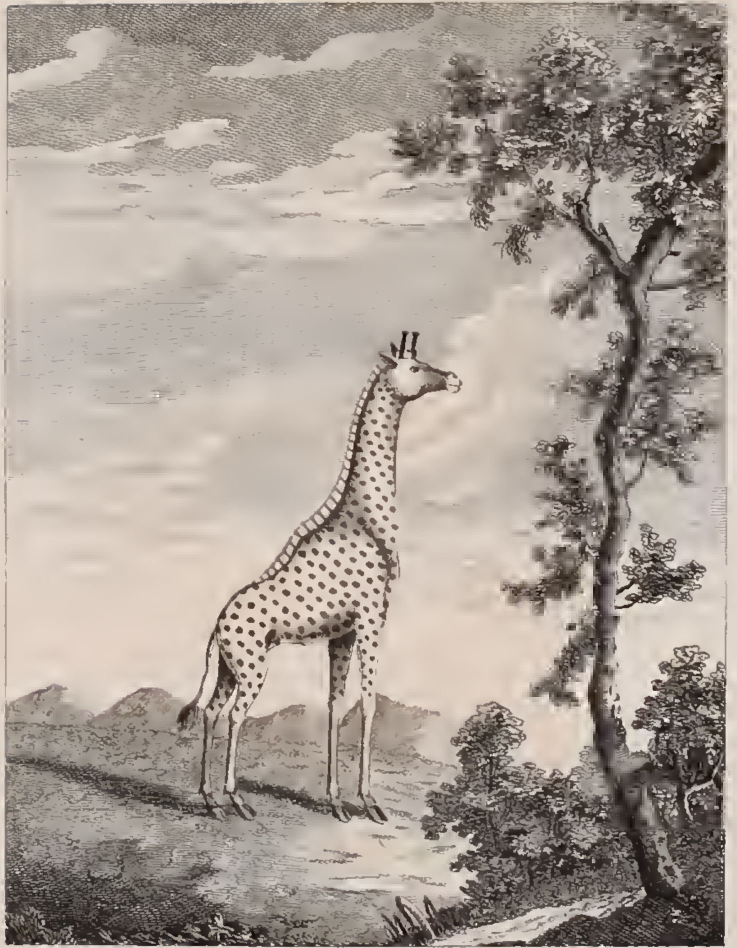
* The animal deſcribed in this letter is now in the cabinet of Natural Hiſtory at Leyden, where I have ſeen it this year.

M. Maty.



J. Mynde sculp.

Dimenſions of a Male Camelopardalis, *killed in a journey made in the year* 1761, *through the country of a tribe of* Hottentots, *called the* Mamacquas, *viz.*

	feet	inches
Length of the head,	1	8.
Height of the fore-leg from the lower to the higher point,	10	
From the upper part of the fore-leg to the top of the head	7	
From the upper part of the fore-leg to the upper part of the hind-leg,	5	6
From the upper part of the hind-leg to the tail,	1	6.
Height of the hind-leg from the upper to the lower part,	8	5

Received November 9, 1769.

IV. *Experiments in Support of the Uſes aſcribed to Ganglions of the Nerves, in* Philoſophical Tranſactions, *Vol.* LIV. *and Vol.* LVII. *by* James Johnſtone, *M. D.*

Read at R. S. Feb. 1, 1770.

HAVING before had the honour of having my thoughts on the uſes of ganglions, which hitherto had remained an unſolved problem, read to the Royal Society; it was obſerved that a principal, and indeed almoſt the only objection of any weight, to which the doctrine was liable, aroſe from the indiſputable fact, That each of the nerves ariſing from the ſpinal marrow had a ganglion.

The concurrence of argument in favour of the general doctrine perſuaded me, that theſe ganglions of the ſpinal nerves reſpected excluſively the filaments which are detached from the ſpinal nerves to the great ſympathetic nerves, a new contexture taking place in thoſe ganglions capable of intercepting the uſual powers of volition: and that thoſe filaments diſtributed to the voluntary muſcles, underwent no ſuch change from their uſual rectilineal and parallel direction and arangement. I was not, till lately, capable of proving this by experiments, with which I beg leave once more to trouble the Royal Society, as they much ſupport the new uſes aſſigned to ganglions, in the Eſſay publiſhed in the LIVth volume of Philoſophical Tranſactions, and defended in another paper publiſhed in the Philoſophical Tranſactions, volume LVIIth, for 1767, and open a new field of anatomical reſearch and philoſophical ſpeculation, not without ſome proſpect of advancing the practice of medicine in nervous diſeaſes, with the knowledge of the nervous ſyſtem.

But before I proceed to relate the experiments which follow, I beg leave to premiſe what is allowed by all phyſeologiſts, that when any nerve is irritated, the muſcle it goes to, or, if it has a large nerve, or bundle of nerves, all the muſcles ſupplied from thence, are by that irritation violently convulſed.

If then it ſhall appear, that irritation and injuries of the ſpinal marrow, from which the intercoſtal nerves, which ſupply the heart and inteſtines, truly ariſe, no leſs than the nerves diſtributed to the limbs, occaſion violent convulſions of the limbs, and yet do not in the leaſt affect the motions of the heart, and inteſtines, parts the moſt irritable of any in the body, as will indeed appear to any one who repeats the following experiments:

The difference will probably be accounted for by the intervention of ganglion after ganglion, conſtantly found betwixt the ſpinal marrow, and the heart and inteſtines. And the unprejudiced inquirer into nature will probably be led to aſcribe thoſe uſes to ganglions, which I have done.

Experiment 1ſt. On the 4th of March, 1767, a kitten a week old, had its head cut off betwixt the firſt and ſecond vertibre of the neck; the thorax was opened with all expedition, and the heart laid bare to view and obſerved for ſome time, that any difference might be eaſier and more certainly noticed. After the animal ceaſed to move its limbs, I touched the ſpinal marrow with a probe, immediately the extremities of the animal were all ſtrongly convulſed; but the heart alone ſeemed unaffected, and continued to move without acceleration, or any degree of alteration whatever.

Betwixt this time, and the 10th of April, I repeated the ſame experiment upon half a dozen kittens ſtill younger than the firſt; upon opening the thorax, the heart beat near 70 ſtrokes in a minute.

When the heart beat only 40 in a minute, or thereabouts, I began gently to touch the ſpinal marrow with the point of a probe, and the limbs were immediately convulſed, but the heart not in the leaſt affected.

I ſlit open both the ventricles of the heart, ſo as to let out all the blood they contained; and inſtantly the heart ceaſed to beat (though its pulſation continues otherwiſe very long in animals ſo young): but the auricles which were not opened, and therefore were ſtill ſtimulated with the blood, beat on.

In this ſituation, in ſeveral of theſe animals, I thruſt the probe into the ſpinal marrow; but the heart neverthelеſs continued in perfect reſt, and inaction; though, when its ſubſtance was pricked with the point of a knife, it might ſtill be made to contract.

But though the heart and inteſtines remained equally unaffected in all the tryals I made by thruſting the probe into the ſpinal marrow, the following contractions occaſioned by it deſerve particular enumeration.

All the limbs were violently convulſed.

The muſcles of the neck were convulſed, and the ſpine bent as in the opiſthotonos.

The intercoſtal muſcles were all contracted; and their natural action (that of drawing all the ribs nearer each other and the ſummit of the thorax) rendered matter of ocular demonſtration.

The diaphragm was contracted ſtrongly, notwithſtanding the phrenic nerve of one ſide was divided in making ſimilar experiments, by pricking and ſtretching it; which, by the way, conſtantly occaſioned a convulſive contraction of the diaphragm.

Even by plunging the probe into the brain, after the head had been cut off for ſome minutes, the tongue, and ſome other muſcles were made to contract.

But theſe experiments muſt be made in leſs than a quarter of an hour after decapitation; half an hour afterwards no ſuch effects follow the deſtruction of the ſpinal marrow. And they ſucceed beſt by previouſly opening the ventricles of the heart.

Experiments ſimilar to theſe have been made upon frogs, by Stuart, Baron Haller, and Dr. Whytt, and others. "When I opened" (ſays the laſt of theſe ingenious gentlemen) "the thorax of a frog, immediately after decollation and deſtroying its ſpinal "marrow, I obſerved its heart beating at the rate of "60 in a minute, which is four or five pulſations

" leſs than I have generally ſeen the hearts of frogs " make, in that time, when their thorax was opened " without decollation." — (See Whytt's Exp. on living and dying animals. Eſſ. Ph. and Litt. vol. II. p. 282. – alſo Whytt's Phyſ. Eſſays.)

II. " Some young gentlemen having hanged a " cat, till ſhe was quite dead, opened the thorax, " and obſerved only a tumultuous motion in the " heart; which ſoon ceaſed, but was renewed by " pricking it with a ſharp inſtrument; after this, " by ſqueezing the cardine nerves downwards, or " otherwiſe irritating them, the heart was made to " perform two or three pulſations, which it con- " tinued to do for a conſiderable time whenever the " cardine nerves were thus ſtimulated." (Whytt's Vital Motions, p. 355.) I relate theſe experiments in the words of their excellent author, as they were made with no view to the doctrine I ſhall endeavour to ſhew they enforce and ſupport.

III. Animals are killed ſome ſooner, others, eſpecially of the cold kind, (as frogs and tortoiſes, probably on account of the largeneſs of their ſpinal marrow) much later, by cutting through the ſpinal marrow near its origin, by which the chief communication of the cardine nerves with the brain is cut off.

The cutting through the intercoſtal or great ſympathetic nerves, or the tying ligatures upon them, is alſo ſooner or later ever fatal to the animal the experiment has been tried upon; by finally deſtroying the motion of the heart, and inſtantly wonderfully weakening and diſturbing its motions. See Vieuſſens.

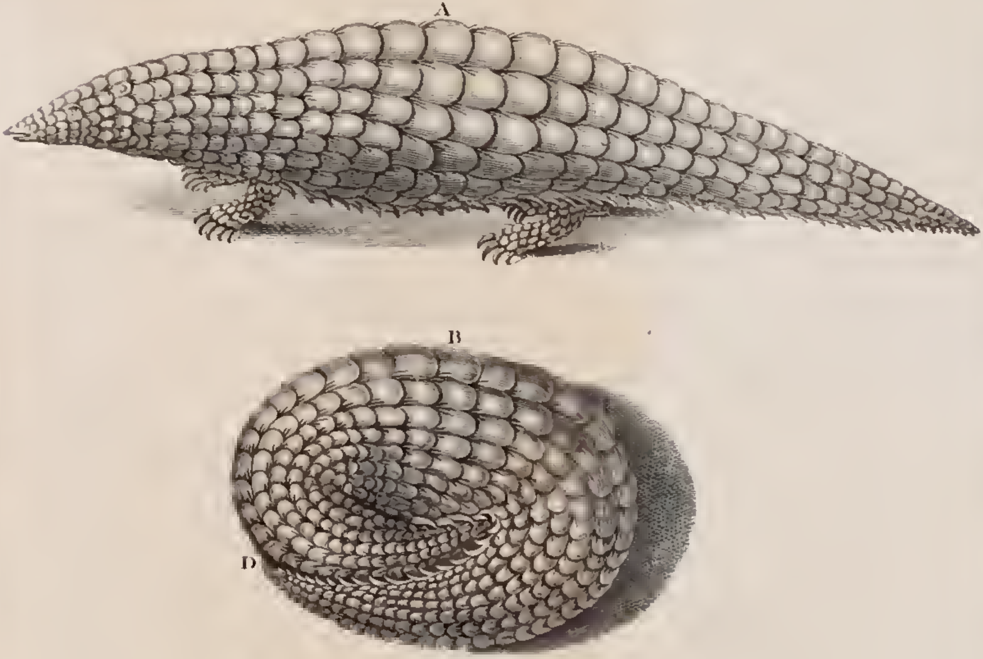
Theſe experiments ſeem to prove, that the Ganglions on the ſpinal nerves do not hinder the irritation of the ſpinal marrow from cauſing convulſions in the voluntary muſcles; and that the ſame Ganglions, (Exp. I.) do in all probability hinder that cauſe from acting (as without their intervention it muſt have done) upon the heart by means of its nerves chiefly ariſing from the ſpinal marrow originally; and therefore it ſeems evident, and beyond a plauſible conjecture, that the Ganglions on the ſpinal nerves relate excluſively and ſolely to the intercoſtal, or as they are otherwiſe called, the great ſympathetic nerves, for the purpoſes I have formerly endeavoured to prove: and this derives farther confirmation from experiment II. as we thereby ſee that the heart may be made to move, as all other muſcles may, by irritating or ſqueezing its proper nerves below their ganglions; and that the motions of the heart cannot long continue, in warm animals eſpecially, III. after the diviſion of its principal nerves, which ſhews the dependance of the motions of the heart, ultimately, as that of all other muſcles, upon its proper nerves, and their connexion with the brain.

V. *An Account of a new Species of the Manis, or Scaly Lizard, extracted from the* German *Relations of the* Daniſh *Royal Miſſionaries in the* Eaſt Indies, *of the Year* 1765, *publiſhed at* Hall, *in* Saxony, *by Dr.* Hampe, F. R. S.

Read Feb. 6, 1770.

" OCTOBER the 15th, yeſter eve-" ning a rare and remarkable animal was in the city of Tranckebar diſcovered in " the wall of an oil merchant's houſe, and with " difficulty killed. The Mallabars call it *Alungu*. " It ſomewhat reſembles a large lizard, except the " head and tail, which are alike as to ſhape, being " both pointed, the former not unlike a mole's. " The whole length is a German ell and five eighths " long, and its breadth half an ell. The tail is " half an ell long, and its broadeſt part a ſpan wide. " The fore feet are a quarter of an ell long, the hind " ones a little longer. The ſnout is an eighth of an " ell long, and the extremity of it a thumb in thickneſs. The whole body, excepting under the belly " (where it is ſmooth for about the length and " breadth of a man's hand), and under the feet, is " covered with hard, ſtrong, ſharp, and bright ſcales, " ſhaped like a muſcle-ſhell, the largeſt of which " are of the length and breadth of three fingers.

MANIS manibus pentadactylis, plantis tetradactylis.



A. அழுங்கு, *ALUNGU.* B. *ALUNGU, shews the Animal roll'd.*

C. *Is the Snout.* D. *The Point of the Tail.*

J. Mynde sculp.

" Under its ſcale come out two or three hairs like " hog's briſtles. On its fore-claws are five ſtrong " long nails, on the hind-claws but four. When purſued, it rolls itſelf ſo together, that nothing but the " back and tail are to be ſeen. It could not be " killed, though ſtruck with wooden poles armed " with iron, with which rice is ſtamped; but the " blows on the ſcales brought forth ſparks of fire " from the iron. It was at laſt killed by a ſtroke " under the belly with an iron hook. What is remarkable of this little animal is, that it is able to " kill an elephant, by twiſting itſelf about that " large animal's trunk, and ſqueezing it with its " body and tail (on the ſides of which are rows of " pointed ſcales) ſo long, that it kills the elephant. " This animal is ſeldom ſeen, except in large " valleys."

Thus far the account from the reverend miſſionary Dame, who has likewiſe given us the preſent figure Tab. II.; the animal, from which it is taken, is diſtinguiſhed by the number of the toes, from the hitherto known ſpecies: of theſe there are two, one of them covered with hair, the other with ſcales. The firſt is called by Linnæus Myrmecophaga, and the other Manis. Linnæus, in his 12th edition of the Syſtem of Nature, has but two ſorts of the latter ſpecies; the firſt has five toes (Manis pentadactyla) the other four (Manis tetradactyla) on both hind and fore feet. Between theſe two ſorts, the preſent Alungu is to be placed, having from the firſt the fore-feet and from the latter the hind-feet. This animal belongs therefore to the family of ant-eaters, which have no teeth, but have a long and round tongue, with which they catch the ants.

Whether the tail of this animal is eaten, as aſſerted of the other, Mr. Dame has not been able to find out.

H. Hampe.

Received November 9, 1769.

VI. *An Account of the Reſult of ſome Attempts made to aſcertain the Temperature of the Sea in great Depths, near the Coaſts of* Lapland *and* Norway; *as alſo ſome Anecdotes, collected in the former. By* Charles Douglas *Eſquire,* F. R. S. *then Captain of his Majeſty's Ship the* Emerald, Anno 1769.

Read Feb. 8, 1770.

MAY the 12th, 1769, between the iſlands of Surey and Hammerfeſt, in Lapland, in the latitude of 70° 40′, between the hours of 6 and 9 p. m. the thermometer ſtood in the open air at 27, in the ſea at the ſurface 36, and in three ſeveral depths, from 87 to 78 fathoms at the bottom, as often tried, at 39.

May 17th, in nearly the ſame place, the thermometer having at noon ſtood in the open air at 44, ſtood between 7 and 9 p. m. at 38, at the ſurface 37; and at the bottom in the depths of 86 and 90 fathoms, having been twice tried, at 39.

May 22d, in lat. 70° 32′, between the iſland of Hammerfeſt and the main land of Finmark, about

7 p. m.

7 p. m. the thermometer ſtood at 40, in the open air, in the ſea at the ſurface at 37; and at the bottom in 80 fathoms depth, at 39.

June 29th in the afternoon, between the iſland of Maggeroe and the main land of Lapland, in lat. 70° 54′, the thermometer ſtood in the open air at 47 in the ſea, at the ſurface at 44, and in 98 fathoms water, at the ground at 40.

July 7th at ſea, about 6 leagues diſtance from the iſland of Tromſound, in the lat. of 70° 45′, the thermometer in the open air at 46, in the ſea at the ſurface 46, and at the bottom 70 fathoms deep 44.

July 8th, in lat. 68° 43′, at the diſtance of 12 or 14 leagues from the iſland of Lofoot, in the province of Norland, the thermometer ſtood in the open air at 46, in the ſea at the ſurface 47, 260 fathoms below the ſurface, but not at the bottom, at 52: and 100 fathoms below the ſurface at 46.

July 9th, in lat. 65° 25′, the thermometer in the open air at 48, in the ſea at the ſurface 48 – 210 fathoms deep upon the ground at 48: and 100 fathoms below the ſurface at 46. N. B. The ſhip was then by reckoning about 20 or 25 leagues from the neareſt part of Norway.

July 10th, in lat. 64° 40′, about 30 leagues from the coaſt of Norway, the thermometer in the open air, and in the ſea at the ſurface at 52, at the ground in 141 fathom water, at 46 and 75 fathoms below the ſurface at 45.

The foregoing thermometrical experiments, made in deep water, were effected by means of a tin cylinder, containing a large quart, with an apparatus therein, ſo contrived, as to keep the thermometer ſtanding upright in the middle thereof, without touching its ſides: thus encloſed in a caſe, filled with water from along-ſide, and covered with a cap, ſo as to be perfectly water-tight, I ſunk it with the deep-ſea ſounding-lead; letting it hang juſt clear of the ground for the ſpace of half an hour, and then had it hauled up as briſkly as poſſible, and the caſe being inſtantly opened, I inſpected the thermometer. I found the inconvenience however of making the experiment in this way, becauſe of the length of time thereto neceſſary; wherefore I made a very ſmall hole in each end of the cylinder, whereby to let the water in and the air out, and ſent it down empty, to the end that it might fill as far below the ſurface as poſſible, ſuffering it however always to hang a few minutes, that it might ſurely be full before I cauſed the boat's crew to begin hauling it up. The lead, with this apparatus faſtened to the line a little above it, ſunk 260 fathoms in 3 ½ minutes, and was hauled up in 13 ½.

During my ſtay in Lapland, I made all the enquiry poſſible as to the exiſtence of the aquatic animals, called Kraakens, whoſe dimenſions (according to Pontoppidan) appear to me to be far beyond the ſcale of nature; but I never met with any perſon who had either talked with, or heard of, any one living, who had ſeen any ſuch monſters; on the contrary, the moſt intelligent ſaid, they believed ſuch never exiſted otherwiſe than in imagination. But with regard to the Stoor Worms (which I have oftener heard called Sea Worms by the Norwegians), thoſe who totally diſcredited the exiſtence of the Kraakens told me, they believed them really to exiſt: and a few days before I left the North Cape, the Daniſh miſſionary of Porſanger diſtrict did me the favour, cloſely to interrogate the maſter of a Norwegian veſſel, who appeared to me to be by much the moſt knowing man in his ſtation I had met with in Lapland, as to thoſe ſtupendous worms, as they are called. He ſaid, that about ſix years before, he had ſeen three of them at once off Bergen, floating upon the ſurface of the ſea, twelve parts of the back of the largeſt appearing above water; each part being in length about ſix feet, with the intervals of the ſame length, ſo that upon the whole he judged the animal could not be leſs than twenty-five fathoms long, and about one in thickneſs. He did not pretend to aſcertain the dimenſions of the other two, further than their being ſmaller than the one thus imperfectly deſcribed, and added, that four years before he ſaw thoſe laſt, he had (near the ſame coaſt) ſeen a large one, but could ſay nothing particular as to its ſize. What degree of credit is due to this man's account, I ſubmit to the judgement of the learned Society.

After much enquiry, I could learn nothing ſatisſfactory touching the famous Whirlpool (called by the Norwegians and Dutch the Maal Stroom) lying between the iſlands of Lofoot, until I met with this intelligent perſon, who gave me ſome account thereof, in ſubſtance as follows; viz. That at high water it is perfectly ſmooth and ſafe to paſs over; but as the tide, either ebb or flood, gathers ſtrength, it becomes in proportion exceedingly agitated and dangerous, which extreme agitation and whirling, I preſume, muſt be owing to the unevenneſs of the rocky bottom, over which the current rolls with vaſt rapidity, being confined in a narrow paſſage; for this Norwegian told me, that at very low water, ſharp pointed rocks, reaching then above the ſurface, have been ſeen between the iſlands above-mentioned. No wonder then, that ſuch veſſels may have been turned upſide down, as may have been drawn by the tide, in its moſt rapid ſtate, into this gulph. The ſimple agitation of the water would ſufficiently account indeed for the loſs of open boats. Imperfect as it is, in my humble opinion, this account, if true, which I believe it to be, unravels in ſome meaſure, the myſtery of the Norwegian whirlpool; which I however regret, not having myſelf, conſiſtently with my orders, had it in my power minutely to examine.

The foregoing is, with great deference and reſpect, preſented, by the Royal Society's

Moſt humble

and moſt obedient ſervant,

Cha. Douglas.

N. B. The thermometrical experiments in queſtion were made at the ſuggeſtion of Mr. Wilſon, Profeſſor of Aſtronomy at Glaſgow, with a thermometer of his making.

OBSERVATIONS of the TEMPERATURE of the WEATHER, Taken on board his Majeſty's Ship the Emerald, in LAPLAND, with a Thermometer upon Fahrenheit's Scale, Anno 1769.

South Eaſt of Maggeroe Iſland,
in Lat. 71° 00′.

At Sea.

In Hammerfoſt Harbour,
in Lat. 70° 40′.

	April						May																										
ours	25	26	27	28	29	30	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27
.m.	48	47	45	49	51	49	40	24	24	24	23	30	31	34	30	44	41	34	38	42	32	34	44	44	44	42	44	48	32	39	38	39	39
							39	23			25		30	32																			
		46				48		22			27			31		42																38	
	47		44	48	49		38				29		29	30																	39		
							36		25	23	26	28			29	38	40		39	40			42					46					
		44	43			47		23		22	25							30				32				40	42			42			40
	45			47	47		32	22					28	29				27					38	40	42								
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		41	41	46	46		31		29	19				28		36																39	
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		39			37	37	27	20		15			26		28		39																
	38		40	39						19								30		3[illegible]	30		38			38		40	40				
.m.		37			36				33										36								46				42	40	
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		41		41			24						29				36		40			33	42					34				39	39
	4		46		45	45	23		26	24	25	28	30		32	40				32	31	34	44						40				
				44			22	25	25		26							32					46		41	42		32					38
	47	44	48	48	49	47	23				28	30		30	34		33		42		32	38	47	46			50				39		
		45		50	4	49	24		24		30	32	32		38	41		36	44		33	44	45			44			39	32			

VII. *De modo marmoris albi producendi, Diſſertatio epiſtolaris Domino* Maty, *Societatis Regiae* Londinenſis *Secretario, auctore* R. E. Raſpe, *Sereniſſ.* Haſſ. *Landgravio à Conſiliis, S. Reg. Sodali.*

Read Feb. 8. 1770.

RECENTI haeret memoria & Societatis Regiae me cooptantis collocatum beneficium, & promiſſum ea occaſione a me datum, me de modo marmoris albi producendi, ubicunque natura materiem praebuerit, tecum collocuturum.

Digna certe res, quae doctorum hominum ſubjiciatur examini & a peritis in artium vertatur commodum.

Obſervavit ab aliquo inde tempore Rev. Abbas Vegni, fundi vel dominus vel poſſeſſor, thermarum S. Philippi fontes ad Radicofanum in Etruria ſitos, ad viam, qua Florentia Romam itur. Vidit 1°, de colle parum edito, & qui totus ex marmore candido compoſitus videtur, diverſis rivulis defluentes. 2°, Sulphure abundare didicit ex floribus ſulphuris aquae innatantibus; & de virtute medica quaedam ni fallor legiſſe me memini in Targioni Tozzetti itinere litterario per Toſcanam inſtituto. Quod vero magis ad rem eſt, vidit, 3°, frequentiorem deponentes tophum candidum atque nitentem eoque uti rivulorum & canalium latera ita quaelibet his injecta corpora dura incruſtantes, & ſic quidem ut tophus ille exactiſſime impreſſam ſervet imaginem & formam incruſtatorum corporum, ſi feliciter rumpi contingat. Rivulis autem iſtis aut topho cumulato aut alio caſu obturatis exundare, 4°, vidit aquas, & quem ante in ipſis canalibus depoſuerant tophum jam deponere in quocunque novo altiore jam defluant alveo.

Ea omnia viro ingenio potenti duplicem moverunt ſuſpicionem, felicem utramque; primam, totum iſtum thermarum collem non eſſe niſi ſucceſſive ab aquis exundantibus cumulatam molem tophi hujus candidi atque nitentis, nam natura exactiſſime conveniunt, praeter quod tophus noviter depoſitus aliquantum mollior eſſe ſoleat interiore collis lapide; alteram, tophi iſtius praecipitationem artibus inſervire poſſe, ſi formis capiatur.

Felices dixi haſce viri conjecturas, & tales revera dici merentur. Quod ad primam enim, ea mirum quam belle, non ex ingenio, ſed facto petitam, rationem reddat originis collis non ſolum Radicofanenſis, verum etiam, quod mihi addere liceat, originis lapidicinarum Marmoris candidi.

Illae nunquam occurrunt in montibus ſtratorum regulam ſervantibus, ſed qui toti conſtant ex rudi, informi, indigeſtoque ſolido ſaxo, nullum plantarum aut animalium marinorum continente veſtigium, ex ſucceſſiva appoſitione facillime explicando, non conveniente cum reliquis hucuſque de origine montium datis hypotheſibus. Pleraeque etiam marmoris candidi ſpecies per cryſtalliſationem quandam coaluiſſe cuilibet inſpectanti videntur; & quae incluſa marmoris ſaxis ſolidis nonnunquam cum hominum ſtupore inventa fuere ferramenta arte facta, clavi, cunei, dolabrae, mallei caedendis aut poliendis lapidibus inſervientia, ea jam non lapidum quandam vegetationem aequivocam, qualis Tournefortio, Baglivio credo etiam Stenoni animo haerebat, produnt, ſed quod res eſt ſaxi olim fluiditatem & ſucceſſivam appoſitionem.

Raro naturam in facto deprehendere licet. Semel deprehenſam alteram Protea non dimittamus, antequam & de aliis ſuis operibus reſponſa dederit. In memoriam igitur tibi revoces velim montes alabaſtriferos, gypſeos, tophaceoſque. Quidni & hos a fontibus olim exundantibus & tophum, pro molecularum natura diverſum, deponentibus originem habuiſſe ſtatuamus? Natura, quae marmoris candidi ſaxis in permultis ſimiles creavit, tantum abeſt ut impediat, ut magis ipſa eandem originem illis eſſe indicet, quam Radicoſani tophis eſſe confeſſa eſt.

Vidi egomet complures alabaſtri & gypſi fodinas, Oſterodae ad fauces Hercyniae, Ronnebergae prope Hannoveram, Weentzae in praefectura Lanenſteinenſi & Morſihae in ripa Fuldae aliquot lapidibus Caſſellis diſtante loco. Nullum unquam in his regulae ſtratorum aut corporum marinorum veſtigium. Fluxiſſe vero materiem & irriguiſſe omne mihi praedicabat ſaxum; imo, quod ipſum majorem cum Tophis & aquis Radicoſanenſibus indicat ſimilitudinem, pleraeque alabaſtri & gypſi ſpecies, cum calcinantur, ſulphur odore produnt, quod nativum & pellucidum ineſt gipſi minerae Weentzeenſi, ex micis & lamellis glaciei Mariae candidiſſimis compoſitae.

Quod

Quod ad montes tophaceos attinet, illi per omnes provincias calce abundantes occurrere ſolent, frequentiores prope Gottingam & Jenam & ubicunque aquae tophum deponunt. Nec in his ullum ſtratorum aut corporum marinorum veſtigium ; ſaepiſſime autem illis incluſae leguntur plantarum terreſtrium imagines & concharum fluviatilium teſtae. Quam maxime memorabilem hujuſmodi collem, & abſque Radicofanenſis autoritate, reliquorum ſimilium originem prodentem mihi indicavit excellentiſſimus liber Baro de Waitz, Sereniſſimi Haſſiarum Landgravii a Conſiliis intimis. Situs is eſt in comitatu Schaumburgenſi, & habet ſontem incruſtantem de vertice defluentem ; intra hominum vero memoriam altitudini ejus aliquid adjectum ex certiſſimis geometrarum obſervationibus & accolarum teſtimoniis conſtat, occultavit etenim turris cujuſdam haud procul diſtantis cuſpidem, olim geometris in vicina planitie conſtitutis conſpicuam, & proinde pro objecto fixo atque conſtante uſitatam, cum chartam regionis geographicam delinearent aliquo abhinc tempore ad veteres menſuras exactam & emendatam.

Hiſce novis montibus ſuo & optimo quidem jure jungo, quos ad mare Caſpium, prope antiquiſſimam Perſarum urbem Baku, a ſe viſos deſcripſerunt Kaempfer in Amoenitatum exoticarum faſciculo, N. p. 280. & 282. & D. Lerche in Itinerario recentiſſime edito in Buſchingii collectionis geographicae, quam vernacula lingua Geographiſches Magazin inſcripſit. tomo III. Quos Lerchius noſtra memoria vidit aliquot dierum itinere ab urbe ſupradicta diſtant, ſiti ad pagum Nawagi, & tractu plus quam quindecim werſtarum currunt. Lerchii autem verbis, cum quibus Kaempferiana in praecipuis conſpirant, deſcribam: "Hic vidi, inquit, montium creſcentium miraculum. "Proximi viae altitudine vix caſam ruſticam, remo- "tiores turrem aequabant. Colore ſunt cinereo, "ſteriles, ſine ullo gramine inhoſpitales, figura coni- "formi. Solum limus ſalſus. In vertice fons aquae "ſalſae, paludoſae, turbidae, nonnunquam quaſi "ebullientis atque exundantis, quo facto ſedimen- "tum vertici appoſitum ſiccatur & continuo altiores "reddit. Fontibus igitur hiſce perpetuo mananti- "bus hoſce etiam colles annua & perpetua habere "incrementa in aperto eſt. Vidi tamen nonnullos "plane exſiccatos conſpicuae altitudinis in vicinia "aliorum noviter ortorum vertice adhuc aquoſo & "exundante. His veſtigia caprarum ſylveſtrium ſale "allectarum frequentiſſime impreſſa viſuntur. Ac- "cepi poſtea autumnali & brumali tempore, quo "pluviae abundant, largius & copioſius defluere. "Tribus etiam a via lapidibus diſtans montium "jugum vidi, de quo limus iſte ſalſus per latum "terrae tractum decurrebat. Metu infidi ſoli atque "nimis paludoſi territus propius accedere non ſum "auſus; altiorum etiam montium vertices pro- "fundiſſimos nunciabant fontes."

Haec ſufficiant ad illuſtrandam Vegńii ex thermis Radicoſanenſibus deductam conjecturam à me latius & de diverſorum aliorum in continenti montium nova origine expoſitam; quae factis & obſervationibus fundata mihi quidem divinam illam mediorum naturae ſimplicitatem, ejuſque in hiſce mediis divitias novis argumentis firmare, imo haud ſpernendum hiſtoriae naturalis, & ejus quod aliquot abhinc annis Societati Regiae inſcripſi, ſpeciminis de novis inſulis & montibus augmentum videtur.

Venio ad uſum, quem tophi Radicofanenſis praecipitati Abbati Vegnio promittere videbatur, & re vera praeſtitit. Tam proſperum vero ſucceſſum habuere prima ab illo inſtituta experimenta, ut officinam hujus generis tophaceorum operum ad thermas S. Philippi ſuis ſumtibus aperuit, jam laete florentem. Formis operum variorum ſculptilium e gypſo confectis, iiſque vernice probe ſaturatis atque oleo aut alia pinguedine ſubtili illitis, in thermarum alveos impoſitis, colligunt tophum, intra ſex dierum ſpatium ad craſſitiem duarum linearum accreſcentem, quae ad minora opera ſufficit; & ſic fiunt anaglypha ſeu proſtypa (bas-reliefs), tabulae, medalliones, portarum, feneſtrarum, caminorum poſtes atque ornamenta architectonica, quae ſi elegantioris operis ſormae adhibitae fuerint praeſtantium ſculptorum celo facta & ex puriſſimo marmore Carrarienſi elaborata videntur. Minori pretio venire, quam quae ex marmore fiunt & gratiſſimam ornamentorum architectonicorum & ſculptilium materiem praebere pronum eſt indicatu & quilibet videt apud quem ſculpturae ſuus conſtat honos, Ipſe in collectionibus meis beneficio Langhanſiii architecti Wratiſlavienſis, aliquot abhinc menſibus ex Italia reducis, poſſideo hujuſmodi tophaceum Radicofanenſem medallionem, GERTRVDIS. FLANDERENSIS. DVCISSAE. LOTH. ET MARCHIONISSAE. qui nitore & elegantia ſculptoris eximii manum, & pelluciditate, colore candido cereo, granique ſubtilitate & cum acidis efferveſcentia, optimum marmor Carrarienſe mentitur. Nil igitur moror, quam non vidi medallionum Lotharingiae

&

& Etruriae Ducum ſeriem Imperatori, cum Italiam perluſtraret, oblatam.

Supereſt ut in uſus noſtros convertamus inventum in ſe eximium, quod ubique mihi quidem fieri poſſe videtur, ubi in cavernis aut metallifodinis ſtalactitae generantur, aut fontes, thermae & acidulae fluunt, tophum duriorem eumque candidi aut grati coloris deponentes.

Miſi in propinquam Hercyniam nonnullas medallionum formas, ut in cavernis Baumanniana & Schartzfeldenſi ſtalactitis candidi, rigentem undam deſtillantibus, ſubjiciantur. Miſiſſem in thermas Carolinas Bohaemicas, quae tophos varios, piſiformes candidos, alios grani ſubtilitate marmori ſimiles eoſque frequentes deponunt in canalium alveis, ſed deerat cui committerem. An in Hercynia experimentum ſucceſſerit alio tempore ſcies. Et quidni in ipſo imperio Britannico, per omnes terrarum partes late patente, omniumque jam bonarum artium alma matre, tuis & Societatis Regiae auſpiciis eadem experimenta fiant?

Vale, ut literis, & amicis vivas; meque & mea ſtudia Societati Regiae commendare perge. Dabam Caſſellis, 10 Octobris, 1769.

Received November 28, 1769.

VIII. *Account of a very remarkable young Muſician. In a Letter from the Honourable* Daines Barrington, F. R. S. *to* Mathew Maty, M. D. *Sec.* R. S.

SIR,

Read Feb. 15, 1770.

IF I was to ſend you a well atteſted account of a boy who meaſured ſeven feet in height, when he was not more than eight years of age, it might be conſidered as not undeſerving the notice of the Royal Society.

The inſtance which I now deſire you will communicate to that learned body, of as early an exertion of moſt extraordinary muſical talents, ſeems perhaps equally to claim their attention.

Joannes Chryſoſtomus Wolfgangus Theophilus Mozart, was born at Saltzbourg in Bavaria, on the 17th of January, 1756 *.

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* I here ſubjoin a copy of the tranſlation from the regiſter at Saltzbourg, as it was procured from his excellence Count Haſlang, envoy extraordinary and miniſter plenipotentiary of the electors of Bavaria and Palatine:

I have been informed by a moſt able muſician and compoſer, that he frequently ſaw him at Vienna, when he was little more than four years old.

By this time he not only was capable of executing leſſons on his favourite inſtrument the harpſichord, but compoſed ſome in an eaſy ſtile and taſte, which were much approved of.

His extraordinary muſical talents ſoon reached the ears of the preſent empreſs dowager, who uſed to place him upon her knees whilſt he played on the harpſichord.

This notice taken of him by ſo great a perſonage, together with a certain conſciouſneſs of his moſt ſingular abilities, had much emboldened the little muſician. Being therefore the next year at one of the German courts, where the elector encouraged him, by ſaying, that he had nothing to fear from his au-

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" I, the under-written, certify, that in the year 1756, the 17th
" of January, at eight o'clock in the evening, was born Joannes
" Chryſoſtomus Wolfgangus Theophilus, ſon of Mr. Leopold
" Mozart, organiſt of his highneſs the prince of Saltzbourg,
" and of Maria Ann his lawful wife (whoſe maiden name was
" Pertlin), and chriſtened the day following, at ten o'clock in
" the morning, at the prince's cathedral church here; his god-
" father being Gottliel Pergmayr, merchant in this city. In
" truth whereof, I have taken this certificate from the parochial
" regiſter of chriſtenings, and under the uſual ſeal, ſigned the
" the ſame with my own hand.

Saltzbourg,
Jan. 3, 1769.

" Leopald Comprecht,

" Chaplain to his Highneſs in this city."

guſt preſence; little Mozart immediately ſat down with great confidence to his harpſichord, informing his highneſs, that he had played before the empreſs.

At ſeven years of age his father carried him to Paris, where he ſo diſtinguiſhed himſelf by his compoſitions, that an engraving was made of him.

The father and ſiſter who are introduced in this print, are exceſſively like their portraits, as is alſo little Mozart, who, is ſtiled "Compoſiteur et Maitre "de Muſique, agé de ſept ans."

After the name of the engraver, follows the date, which is in 1764; Mozart was therefore at this time in the eighth year of his age.

Upon leaving Paris, he came over to England, where he continued more than a year. As during this time I was witneſs of his moſt extraordinary abilities as a muſician, both at ſome publick concerts, and likewiſe by having been alone with him for a conſiderable time at his father's houſe; I ſend you the following account, amazing and incredible almoſt as it may appear.

I carried to him a manuſcript duet, which was compoſed by an Engliſh gentleman to ſome favourite words in Metaſtaſio's opera of Demofoonte.

The whole ſcore was in five parts, viz. accompaniments for a firſt and ſecond violin, the two vocal parts, and a baſe.

I ſhall here likewiſe mention, that the parts for the firſt and ſecond voice were written in what the Italians ſtile the *Contralto* cleff; the reaſon for taking notice of which particular will appear hereafter.

My intention in carrying with me this manuſcript compoſition, was to have an irrefragable proof of his abilites, as a player at ſight, it being abſolutely impoſſible that he could have ever ſeen the muſic before.

The ſcore was no ſooner put upon his deſk, than he began to play the ſymphony in a moſt maſterly manner, as well as in the time and ſtile which correſponded with the intention of the compoſer.

I mention this circumſtance, becauſe the greateſt maſters often fail in theſe particulars on the firſt trial.

The ſymphony ended, he took the upper part, leaving the under one to his father.

His voice in the tone of it was thin and infantine, but nothing could exceed the maſterly manner in which be ſung.

His father,. who took the under part in this duet, was once or twice out, though the paſſages were not more difficult than thoſe in the upper one; on which occcaſions the ſon looked back with ſome anger, pointing out to him his miſtakes, and ſetting him right.

He not only however did complete juſtice to the duet, by ſinging his own part in the trueſt taſte, and with the greateſt preciſion: he alſo threw in the accompaniments of the two violins, wherever they were moſt neceſſary, and produced the beſt effects.

It is well known that none but the moſt capital muſicians are capable of accompanying in this ſuperior ſtile.

As many of thoſe who may be preſent, when this letter may have the honour of being read before the ſociety, may not poſſibly be acquainted with

the

the difficulty of playing thus from a muſical ſcore, I will endeavour to explain it by the moſt ſimilar compariſon I can think of.

I muſt at the ſame time admit that the illuſtration will fail in one particular, as the voice in reading cannot comprehend more than what is contained in a ſingle line. I muſt ſuppoſe, however, that the reader's eye, by habit and quickneſs, may take in other lines, though the voice cannot articulate them, as the muſician accompanies the words of an air by his harpſichord.

Let it be imagined, therefore, that a child of eight years old was directed to read five lines * at once, in four † of which the letters of the alphabet were to have different powers.

For example, in the firſt line A, to have its common powers.

In the ſecond that of B.

In the third of C.

In the fourth of D.

Let it be conceived alſo, that the lines ſo compoſed of characters, with different powers, are not ranged

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* By this I mean,
The two parts for the violins.
The upper part for the voice.
The words ſet to muſic.
And laſtly, the baſe.

† By this I mean,
The violin parts in the common treble cleff.
The upper part for the voice in the contralto cleff as beforementioned.
The words in common characters.
And the baſe in its common cleff.

ſo as to be read at all times one exactly under the other, but often in a deſultory manner.

Suppoſe then, a capital ſpeech in Shakeſpeare * never ſeen before, and yet read by a child of eight years old, with all the pathetic energy of a Garrick.

Let it be conceived likewiſe, that the ſame child is reading, with a glance of his eye, three different comments on this ſpeech tending to its illuſtration; and that one comment is written in Greek, the ſecond in Hebrew, and the third in Etruſcan characters.

Let it be alſo ſuppoſed, that by different ſigns he could point out which comment is moſt material upon every word; and ſometimes that perhaps all three are ſo, at others only two of them.

When all this is conceived, it will convey ſome idea of what this boy was capable of, in ſinging ſuch a duet at ſight in a maſterly manner from the ſcore, throwing in at the ſame time all its proper accompaniments.

When he had finiſhed the duet, he expreſſed himſelf highly in its approbation, aſking with ſome eagerneſs whether I had brought any more ſuch muſic.

Having been informed, however, that he was often viſited with muſical ideas, to which, even in the midſt of the night, he would give utterance on his harpſichord; I told his father that I ſhould be glad to hear ſome of his extemporary compoſitions.

The father ſhook his head at this, ſaying, that it depended entirely upon his being as it were muſically

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* The words in Metaſtaſio's duet, which little Mozart ſung, are very pathetic.

inſpired, but that I might aſk him whether he was in humour for ſuch a compoſition.

Happening to know that little Mozart was much taken notice of by Manzoli, the famous ſinger, who came over to England in 1764, I ſaid to the boy, that I ſhould be glad to hear an extemporary *Love Song*, ſuch as his friend Manzoli might chooſe in an opera.

The boy on this (who continued to ſit at his harpſichord) looked back with much archneſs, and immediately began five or ſix lines of a jargon recitative proper to introduce a love ſong.

He then played a ſymphony which might correſpond with an air compoſed to the ſingle word, *Affetto.*

It had a firſt and ſecond part, which, together with the ſymphonies, was of the length that opera ſongs generally laſt: if this extemporary compoſition was not amazingly capital, yet it was really above mediocrity, and ſhewed moſt extraordinary readineſs of invention.

Finding that he was in humour, and as it were inſpired, I then deſired him to compoſe a *Song of Rage*, ſuch as might be proper for the opera ſtage.

The boy again looked back with much archneſs, and began five or ſix lines of a jargon recitative proper to precede a *Song of Anger*.

This laſted alſo about the ſame time with the *Song of Love*; and in the middle of it, he had worked himſelf up to ſuch a pitch, that he beat his harpſichord like a perſon poſſeſſed, riſing ſometimes in his chair.

The

The word he pitched upon for this ſecond extemporary compoſition was, *Perfido.*

After this he played a difficult leſſon, which he had finiſhed a day or two before *: his execution was amazing, conſidering that his little fingers could ſcarcely reach a fifth on the harpſichord.

His aſtoniſhing readineſs, however, did not ariſe merely from great practice; he had a thorough knowledge of the fundamental principles of compoſition, as, upon producing a treble, he immediately wrote a baſe under it, which, when tried, had a very good effect.

He was alſo a great maſter of modulation, and his tranſitions from one key to another were exceſſively natural and judicious; he practiſed in this manner for a conſiderable time with an handkerchief over the keys of the harpſichord.

The facts which I have been mentioning I was myſelf an eye witneſs of; to which I muſt add, that I have been informed by two or three able muſicians, when Bach the celebrated compoſer had begun a fugue and left off abruptly, that little Mozart hath immediately taken it up, and worked it after a moſt maſterly manner.

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* He publiſhed ſix ſonatas for the harpſichord, with an accompaniment for the violin, or German flute, which are ſold by R. Bremner, in the Strand, and are intituled, Oeuvre Troiſme.

He is ſaid in the title page to have been only eight years of age when he compoſed theſe ſonatas.

The dedication is to the Queen, and is dated at London, January 8, 1765.

He ſubſcribes himſelf, "tres humble, et tres obeiſſant *petit* "ſerviteur."

Theſe leſſons are compoſed in a very original ſtile, and ſome of them are maſterly.

Witneſs as I was myſelf of moſt of theſe extraordinary facts, I muſt own that I could not help ſuſpecting his father impoſed with regard to the real age of the boy, though he had not only a moſt childiſh appearance, but likewiſe had all the actions of that ſtage of life.

For example, whilſt he was playing to me, a favourite cat came in, upon which he immediately left his harpſichord, nor could we bring him back for a conſiderable time.

He would alſo ſometimes run about the room with a ſtick between his legs by way of horſe.

I found likewiſe that moſt of the London muſicians were of the ſame opinion with regard to his age, not believing it poſſible that a child of ſo tender years could ſurpaſs moſt of the maſters in that ſcience.

I have therefore for a conſiderable time made the beſt inquiries I was able from ſome of the German muſicians reſident in London, but could never receive any further information than that he was born near Saltzbourg, till I was ſo fortunate as to procure an extract from the regiſter of that place, through his excellence count Haſlang.

It appears from this extract, that Mozart's father did not impoſe with regard to his age when he was in England, for it was in June, 1765, that I was witneſs to what I have above related, when the boy was only eight years and five months old.

I have made frequent inquiries with regard to this very extraordinary genius ſince he left England, and was told laſt ſummer, that he was then at Saltzbourg, where he had compoſed ſeveral oratorios, which were much admired.

I am alſo informed, that the prince of Saltzbourg, not crediting that ſuch maſterly compoſitions were really thoſe of a child, ſhut him up for a week, during which he was not permitted to ſee any one, and was left only with muſic paper, and the words of an oratorio.

During this ſhort time he compoſed a very capital oratorio, which was moſt highly approved of upon being performed.

Having ſtated the above mentioned proofs of Mozart's genius, when of almoſt an infantine age, it may not be improper perhaps to compare them with what hath been well atteſted with regard to other inſtances of the ſame ſort.

Amongſt theſe, John Barratier hath been moſt particularly diſtinguiſhed, who is ſaid to have underſtood Latin when he was but four years old, Hebrew when ſix, and three other languages at the age of nine.

This ſame prodigy of philological learning alſo tranſlated the travels of Rabbi Benjamin when eleven years old, accompanying his verſion with notes and diſſertations. Before his death, which happened under the age of twenty, Barratier ſeems to have aſtoniſhed Germany with his amazing extent of learning; and it need not be ſaid, that its increaſe in ſuch a ſoil, from year to year, is commonly amazing.

Mozart, however, is not now much more than thirteen years of age, and it is not therefore neceſſary to carry my compariſon further.

The Rev. Mr. Manwaring (in his *Memoirs of Handel*) hath given us a ſtill more appoſite inſtance, and in the ſame ſcience.

This great muſician began to play on the clavichord when he was but ſeven years of age, and is ſaid to have compoſed ſome church ſervices when he was only nine years old, as alſo the opera of Almeria, when he did not exceed fourteen.

Mr. Manwaring likewiſe mentions that Handel, when very young, was ſtruck ſometimes whilſt in bed with muſical ideas, and that, like Mozart, he uſed to try their effect immediately on a ſpinnet, which was in his bedchamber.

I am the more glad to ſtate this ſhort compariſon between theſe two early prodigies in muſic, as it may be hoped that little Mozart may poſſibly attain to the ſame advanced years as Handel, contrary to the common obſervation that ſuch *ingenia præcocia* are generally ſhort lived.

I think I may ſay without prejudice to the memory of this great compoſer, that the ſcale moſt clearly preponderates on the ſide of Mozart in this compariſon, as I have already ſtated that he was a compoſer when he did not much exceed the age of four.

His extemporary compoſitions alſo, of which I was a witneſs, prove his genius and invention to have been moſt aſtoniſhing; leaſt however I ſhould ininſenſibly become too ſtrongly his panegyriſt, permit me to ſubſcribe myſelf, Sir,

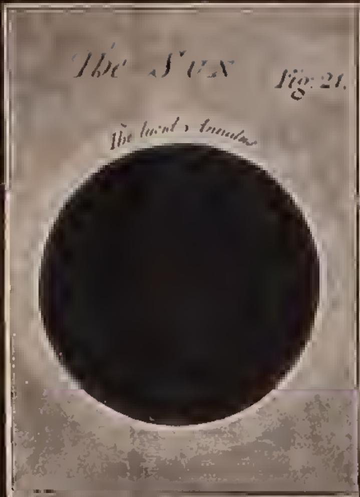
Your moſt faithful
humble ſervant,
Daines Barrington.

Observations of the Transit of Venus over the Limb of the Sun In the Years 1761 & 1769. with the Circumstances & correspondent Apparent Times Referred to the Royal Observatory of GREENWICH, PARIS, &c.

By Samuel Dunn.

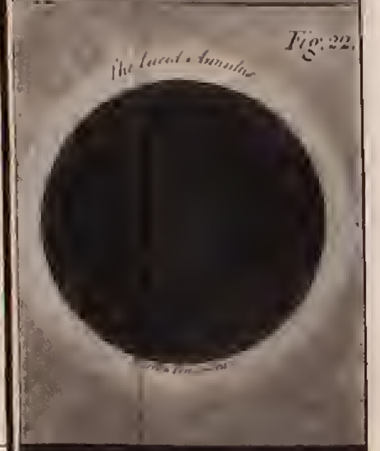
Venus on the Sun June 3d 1769

The SUN Fig. 21.



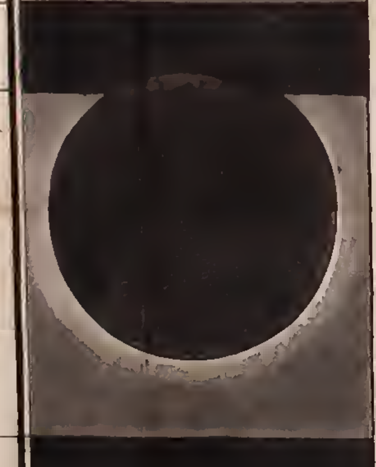
Venus on the Sun June 6th 1761

Fig. 22.



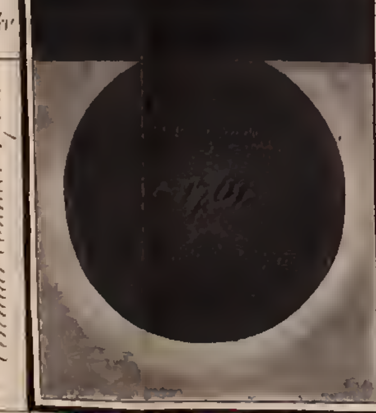
Sky

Circular Contact & Internal 1769



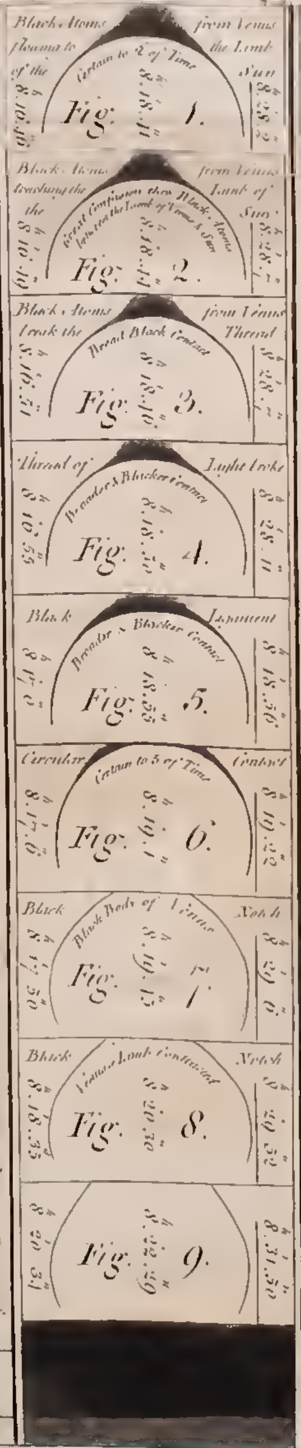
Sky

Circular Contact 1761



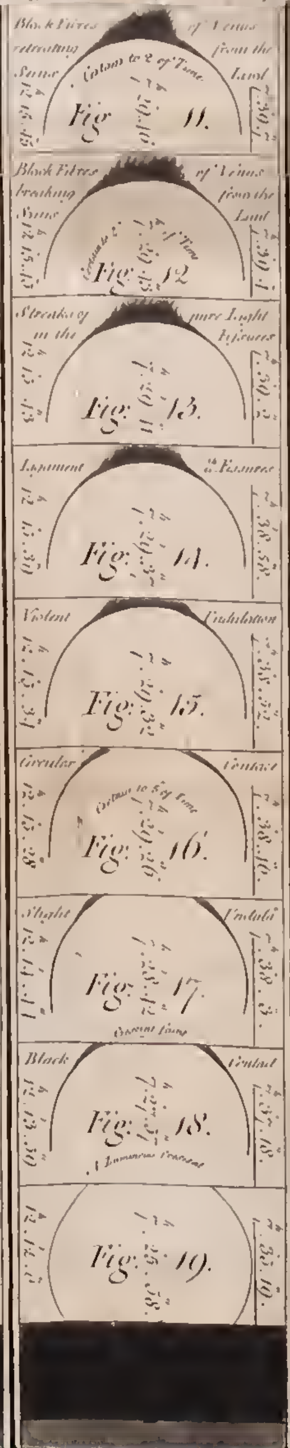
Phænomena in the Year 1761. Sun's Limb

Per Clock | For Greenw.ch | For Paris



Phænomena in the Year 1769. Sun's Limb

Per Clock | For Greenw.ch | For Paris



The Times per Clock for 1761 are the Original Numbers given for Chelsea (see Phil. Trans. 1761.) But those other Times per Clock for 1769 are the Original Numbers shewn by the Clock in the Great Room where the Observation was made in the Royal Observatory at Greenwich. And from which the Apparent Time was had.

Sky

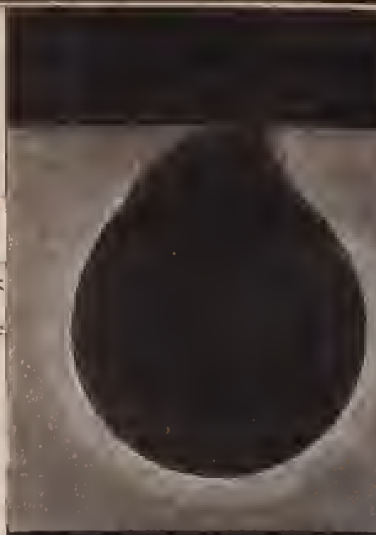
Sun

The Time of External & Internal Contact at both Transits by Telescopes of different Powers, referred to Greenwich.

Internal Contact June 6th 1761 for Greenwich by different Telescopes.	External Contact June 6th 1761 for Greenwich by different Telescopes.	External Contact June 3d 1769 for Greenwich by different Telescopes.	Internal Contact June 3d 1769 for Greenwich by different Telescopes.	Mag. Power
8.18.41	8.37.3	7.10.37	7.29.40	150
8.18.39	8.37.1	7.10.39	7.29.44	125
8.18.37	8.36.59	7.10.41	7.29.41	100
8.18.35	8.36.57	7.10.43	7.29.39	75
8.18.33	8.36.55	7.10.45	7.29.37	50
8.18.44	8.37.3	7.10.37	7.29.43	150
8.18.42	8.37.1	7.10.39	7.29.41	125
8.18.40	8.36.59	7.10.41	7.29.39	100
8.18.38	8.36.57	7.10.43	7.29.37	75
8.18.36	8.36.55	7.10.45	7.29.35	50
8.18.46	8.37.3	7.10.37	7.29.41	150
8.18.44	8.37.1	7.10.39	7.29.39	125
8.18.42	8.36.59	7.10.41	7.29.37	100
8.18.40	8.36.57	7.10.43	7.29.35	75
8.18.38	8.36.55	7.10.45	7.29.33	50
8.18.50	8.37.3	7.10.37	7.29.37	150
8.18.44	8.37.1	7.10.39	7.29.35	125
8.18.46	8.36.59	7.10.41	7.29.33	100
8.18.44	8.36.57	7.10.43	7.29.31	75
8.18.42	8.36.55	7.10.45	7.29.29	50
8.18.55	8.37.3	7.10.37	7.29.32	150
8.18.53	8.37.1	7.10.39	7.29.30	125
8.18.51	8.36.59	7.10.41	7.29.28	100
8.18.49	8.36.57	7.10.43	7.29.26	75
8.18.47	8.36.55	7.10.45	7.29.24	50
8.19.1	8.37.3	7.10.37	7.29.26	150
8.18.59	8.37.1	7.10.39	7.29.24	125
8.18.57	8.36.59	7.10.41	7.29.22	100
8.18.55	8.36.57	7.10.43	7.29.20	75
8.18.53	8.36.55	7.10.45	7.29.18	50
8.19.45	8.37.3	7.10.37	7.28.42	150
8.19.43	8.37.1	7.10.39	7.28.40	125
8.19.41	8.36.59	7.10.41	7.28.38	100
8.19.39	8.36.57	7.10.43	7.28.36	75
8.19.37	8.36.55	7.10.45	7.28.34	50
8.20.30	8.37.3	7.10.37	7.27.57	150
8.20.28	8.37.1	7.10.39	7.27.55	125
8.20.26	8.36.59	7.10.41	7.27.53	100
8.20.24	8.36.57	7.10.43	7.27.51	75
8.20.22	8.36.55	7.10.45	7.27.49	50
8.22.29	8.37.3	7.10.37	7.25.58	150
8.22.27	8.37.1	7.10.39	7.25.56	125
8.22.25	8.36.59	7.10.41	7.25.54	100
8.22.23	8.36.57	7.10.43	7.25.52	75
8.22.21	8.36.55	7.10.45	7.25.50	50
Fig: 10. do	8.37.3	Fig: 20. do	7.10.37	150
	8.37.1		7.10.39	125
	8.36.59		7.10.41	100
	8.36.57		7.10.43	75
	8.36.55		7.10.45	50

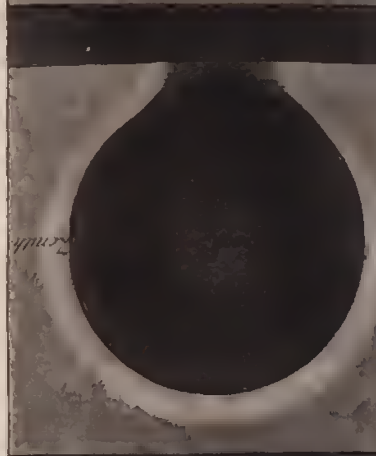
Sky

Thread of Light broken 1769



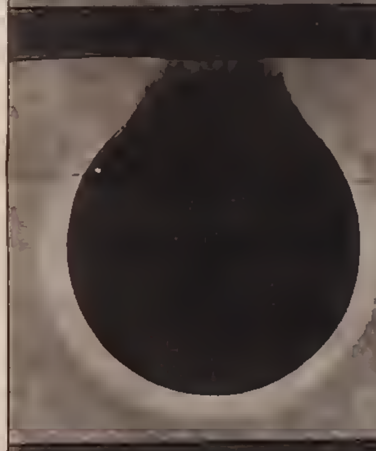
Sky

Thread of Light breaking 1761



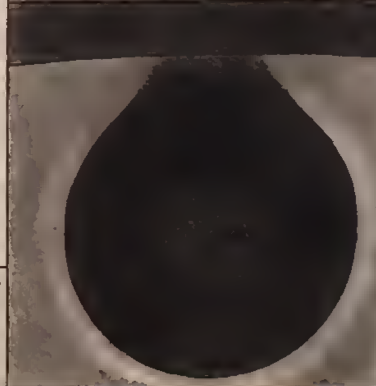
Sky

Thread of Light breaking 1769



Sky

Thread of Light broken 1761



In 1769 from the first additional ferment around the Point of Contact to ye Completion of the Thread of Light was near two Minutes of Time. In 1761 nearly the same time in protuberating: S Dunn

Basire sc.

IX. *A Determination of the exact Moments of Time when the Planet* Venus *was at external and internal Contact with the Sun's Limb, in the Tranſits of* June 6*th*, 1761, *and* June 3*d*, 1769,

By SAMUEL DUNN.

Read Feb. 22, 1770.

IN the year 1761, I preſented to the Royal Society an obſervation which I made of the external and internal contacts of the planet Venus with the ſun's limb, containing ſeveral unexpected phænomena in thoſe contacts, which were ſufficient to render them uncertain ſeveral ſeconds of time. In that paper I took no notice of that lucid annulus which alſo appeared to me to extend outwards from the circumference of Venus to the breadth of about 5 ſeconds of a degree, but was particular in deſcribing a certain narrow wateriſh penumbra near the limb of Venus, which ſeemed not to exceed a tenth or twelfth part of the breadth of the lucid annulus, becauſe it appeared to me of a hazy and indiſtinct colour, and extended itſelf towards the limb of the ſun as the planet approached it, growing more and more dark and broad, ſo that before Venus's real limb ſeemed to be at contact with the ſun's limb internally, the thread of light was broken, though faintly. Another circumſtance was, that the limb of Venus, near the point of contact, left its circular form. But the inſtant of time when the thread of light was broken being the object in view, I omitted a detail of thoſe particulars.

In the late tranſit, June 3d, 1769, I carefully obſerved ſeveral circumſtances attending that phænomenon, which ſeem to me abſolutely neceſſary to be attended to, in order to the exact determination of the time when the internal contact happened; a particular account of them is as follows; viz.

Receiving an invitation from the preſent Aſtronomer Royal to be at the Royal Obſervatory at Greenwich, at the time of the late tranſit, I determined to be there, rather than any where elſe, thinking that the experience which I had had of the former tranſit would enable me to make ſuch an obſervation as might be acceptable to poſterity.

The time came, and the weather proved favourable for making that obſervation, with all the exactneſs that could be wiſhed for or deſired. Several hours before the tranſit, having a very diſtinct idea of the circumſtances which appeared in forming the internal contact of Venus with the ſun's limb, June 6, 1761, and being almoſt certain ſomething of the ſame kind would now happen, I drew out ſmall circular diagrams, repreſenting Venus at ſeveral near diſtances within the ſun's diſc, with the like obſcurity near the point of contact as I had obſerved in the tranſit of 1761, and requeſted Mr. Hirſt, Mr. Dollond, and Mr. Nairne, who were likewiſe to obſerve, to uſe their utmoſt endeavours for determining whether any ſuch impediment occurred or not; and if it did, to note the circumſtances of it.

The clocks to be obſerved by, were ſet by the Aſtronomer Royal, but we were not acquainted with the time they were too faſt or ſlow; and the teleſcope which I obſerved with was an excellent achromatic teleſcope, magnifying 140 times, inverting the object, and made by Mr. Dollond.

The teleſcope being properly adjuſted, and having a moſt clear and diſtinct ſight of the ſun's limb where the external contact was expected to happen; at 11^h 56′ 32″ per clock (which, reduced by the Aſtronomer Royal to apparent time, is 7^h 10′ 33″), whilſt I was moving my eye gently along that part of the ſun's limb where the contact was expected, there appeared as though a kind of lucid wave of tranſparent matter, of the colour of that part of the lucid annulus (which afterwards appeared round Venus), which was neareſt to the limb of Venus, and taking up the ſpace of about a fifth part of a minute of a degree along the ſun's edge, this lucid wave ſeemed to ſtrike gently againſt the ſun's limb, and in an inſtant the little tremulous vibrations on the ſun's limb were totally ſtopt, and that part of the limb was rendered thereby a little obſcure. A ſecond and half of time after this, at the ſame place of the ſun's limb, aroſe, firſt gently, and then more violently, a ferment, or boiling, very different in colour as well as magnitude, from the tremulous vibrations at other parts of the ſun's limb, for it was darker and much more violent, and at 11^h 56′ 36″ per clock, or 7^h 10′ 37″ apparent time, this fermentation was enlarged along the limb of the ſun, and the limb of Venus was entering on the ſun's limb as in Fig. 20. Tab. III.

My attention being at this time engaged in examining the place around the point of contact, I endeavoured to ſee a kind of brown penumbra precede the limb of Venus, but ſaw none; inſtead thereof a kind of whitiſh light, at firſt very faint, and afterwards as it advanced on the ſun's diſc becoming more ſtrong, preceded the limb of the planet; which light gradually diminiſhed nearer to Venus, and formed a narrow margin of lucid matter, whereby the limb of the planet became a little ill defined. Almoſt the ſame circumſtances happened in the exterior contact 1761, but with this difference, the lucid border then following, the limb of Venus was more clear and tranſparent. I was the more particular in theſe circumſtances, to be able to determine what differences might ariſe from obſervations made with teleſcopes and eyes equally good, and conclude from the phænomena, that two ſuch obſervers, with but little inequality in their judgments, might differ from each other ten ſeconds of time. Before this contact and a little after it, I endeavoured to find a faint illumination on the exterior limb of Venus; but could find none, till the time of internal contact drew near. Another circumſtance attending the phænomenon was this; the limb of Venus which firſt entered on the ſun's diſc appeared to be the arch of a very ſmall circle; but as the planet advanced onward on the ſolar diſc, that ſame preceding part of Venus appeared to enlarge and expand itſelf, and the ſubſequent part of Venus which was on the ſun's limb, appeared as though it was the portion of a ſmaller circle; and thus the planet appeared to the time of central ingreſs, at which time half the planet appeared a ſemi-ellipſis, the conjugate diameter forming the notch in the ſun's limb. After the time of central ingreſs, whilſt the latter half of the planet was paſſing over the ſun's limb, the like appearances occurred, ſo that, although that circumference was really concave to Venus's centre, a little after the central ingreſs, it appeared a little convex to that centre, and ſo the planet advanced, that part of it which was neareſt the ſun's limb appearing contracted, but enlarging itſelf a little farther on the diſc.

The planet being conſiderably paſt the central ingreſs, and being at broad black contact with the ſun's limb, but of an irregular form on account of the abovementioned circumſtances, and it being hard to judge what kind of contact would appear, I perceived a very faint luminous creſcent exterior to the limb of the ſun; and nearly coinciding with the preceding limb of Venus continued over the ſun's limb. See Fig. 23. This creſcent was very faint, but ſteadily defined at certain fits and returns till 7ʰ 28′ 30″ apparent time. When this creſcent being come near to the limb of the ſun, it vaniſhed, and ſeemed to fall in with a kind of confuſed ſlight illumination in the limb of the ſun itſelf, where the internal contact was to happen. At the ſame time a kind of partial and very faint illumination took place, both a little without, and a little within the ſun's limb, as well as in the limb itſelf, where the contact was to be, and a gentle ebullition or boiling aroſe a little without the ſun's limb on the limb of Venus, which continued till the dark body of the planet was wholly within the ſun's diſc, or 7^h 29′ 28″ apparent time, when Venus's circumference was not paſſed coinciding with the ſun's circumference above three or four ſeconds of time. Whilſt I was attentively viewing this, and judging it difficult to determine the exact moment of circular contact, on account of the circumſtances deſcribed, the ebullition or boiling between the limb of Venus and the ſun became more violent, and the partial illumination increaſed; and at 7^h 29′ 38″ I ſaw the planet as it were held to the ſun's limb by a ligament formed of many black cones, whoſe baſes ſtood on the limb of Venus and their vertexes pointing to the limb of the ſun. Theſe cones put on various poſitions, and as Venus advanced they alternately contracted themſelves towards the limb of Venus, and expanded themſelves towards the ſun's limb, performing their undulations always regularly and in the ſame time, as the planet advanced on the diſc, till 7^h 29′ 48″ apparent time. At the end of this interval, the agitation or fermentation was exceeding violent, for the whole limb of Venus would ſometimes librate towards the limb of the ſun, and ſometimes the limb of the ſun would turn convex in yielding towards Venus; but the thread of light was not yet formed, for ſtill three or four broad parts of the ligament never had yet broke from the ſun, and therefore the thread of light was not yet formed. I carefully examined the ſides of thoſe black cones connected with the limb of the ſun, and ſaw the fiſſures or ſpaces between them to be filled with a ſteady illumination, of the colour of twilight compared with the light of the ſun; and whilſt I was ſteadily attending to theſe circumſtances, I ſaw the pure and genuine light of the ſun break in between ſome of thoſe fiſſures like ſtreaks of lightening, which made the partial light become in two or three ſeconds of time, of the ſame colour as the light of the ſun, yet ſtill the undulating ligament though reduced was not broken. And now,

In an inſtant, the northern part of the divided ligament withdraws itſelf from the ſun's limb about half way towards Venus, and inſtantly but gently it returns and again unites the limbs of the ſun and Venus; inſtantly after, another leſs northern part of the ligament does the like, and then breaks off again, and ſo doth each part of the divided ligament, till $7^h\ 29'\ 51''$ apparent time, when the ends or vertexes of the black cones between Venus and the ſun's limb appear to be ſeparated from the ſun's limb, retreating to that of Venus, and diſſolving or dying away like a drop of tinge thrown into water, and now the thread of light becomes compleat.

The internal contact being paſt, and Venus being wholly on the ſun, I examined the ſpace ſurrounding Venus, and ſaw ſuch a lucid annulus around the planet as appeared in 1761. The part of this annulus next to the limb of Venus appeared a little duſky, but much more clear than in 1761 (when it appeared more confuſed, and as a penumbra); but that part of the annulus fartheſt off from the circumference of Venus appeared to me a little tinged with blue. The breadth of the annulus about five or ſix ſeconds.

Upon hearing a gentleman in the lower apartment call out to be ſhewed the atmoſphere of Venus, I now left my teleſcope, went down ſtairs to Mr. Nairne and Mr. Dollond, and deſired them to be attentive at their teleſcopes, and they would ſee this ſhining annulus, which they attended to, and after a little while ſaw it plainly, although for ſome time they could not perceive any ſuch thing. Then the other gentlemen preſent ſaw it, but not before me nor them.

Theſe obſervations being made, I ſtate the firſt external contact at 7ʰ 10′ 37″ apparent time for Greenwich. Circular contact internally at 7ʰ 29′ 25″. Completion of the thread of light at 7ʰ 29′ 48″ under the circumſtances above deſcribed.

Although I would not willingly form any hypotheſis from the aforementioned phænomena, there is one of them, namely, the appearance of the well-defined ſtreaks of light between the fiſſures, which ſeems accountable for thus. The partial light which preceded it, I take to be rays ſcattered by refraction and reflection through that part of the planet's atmoſphere where the contact was to happen; and the well-defined ſtreaks of light following it, I take to have been the ſun beams paſſing between mountains on the ſurface of Venus's globe.

To this account I have added ſeveral drawings of the appearances in the tranſit of 1761, and of the like appearances in the tranſit of 1769; and muſt here beg leave to explain the meaning of an expreſſion made uſe of by me in the account of the former tranſit of 1761, in Phil. Tranſ. In that account,

count, by the expreſſion, *no loſs of light*, I mean, that at thoſe times the thread of light was not broken, but only obſcured by the brown or dark penumbra which interpoſed between Venus and the limb of the ſun.

In the drawings, TAB. III. Fig. 1. 2. 3. 4. 5. 6. ſhew the penumbra which interpoſed in 1761, with the coreſpondent times. Fig. 11. 12. 13. 14. 15. 16. are the ſimilar appearances in 1769. Oppoſite to which are the times of thoſe appearances by different teleſcopes, which table of different teleſcopes was formed from experiments. Fig. 7. 8. and 9. 17. 18. and 19. ſhew how much the planet's figure differed from a circle in both tranſits. Fig. 10. ſhews the external contact for 1761, and Fig. 20. the external contact for 1769, and Fig. 21. 22. 23. 24. 25. 26. 27. 28. are more exact repreſentations of the phænomena at the times there mentioned.

Samuel Dunn.

Received November 9, 1769.

X. *An Account of ſome Improvements made in a new Wheel Barometer, invented by* Keane Fitz Gerald, *Eſquire, F. R. S. In a Letter to* Charles Morton, *M. D. Sec. R. S.*

DEAR SIR,

Read Feb. 22, 1770.

I GAVE a former deſcription and drawing of a wheel-barometer of a new conſtruction, with regiſters to mark the riſe and fall of the mercury, which were publiſhed in the firſt part of the fifty-ſecond volume of the Philoſophical Tranſactions for the year 1761. As that iuſtrument has continued to operate effectually by the weight of a few grains only, and the regiſters to mark the limits of the aſcent and deſcent of the mercury in the tube very exactly ever ſince, it has induced me to attempt a further improvement, which I have alſo found to anſwer effectually; and take the liberty of deſiring the favour of you to lay a deſcription and drawing of it before the gentlemen of the Royal Society, if you think a communication of the kind worth their notice.

As

Fig 2

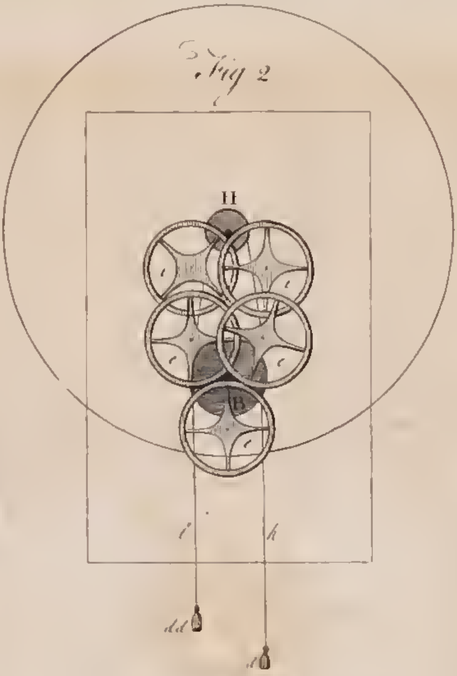
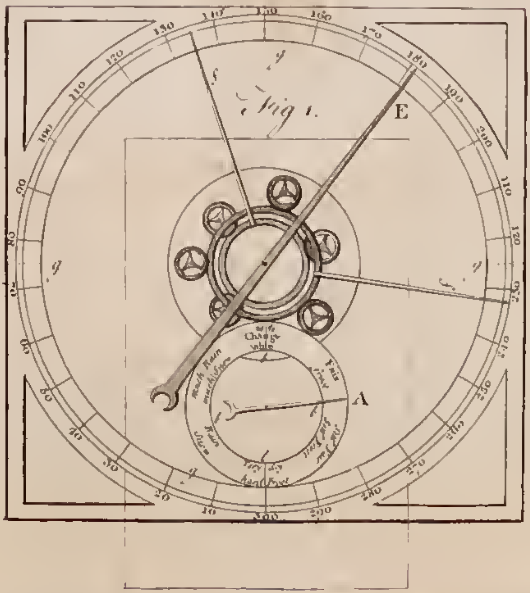


Fig 1.



Basire sc.

As the improved inſtrument is like that already deſcribed, except in the dial-plate, and part of the wheel work, I have made a drawing of the parts, only, wherein they differ.

The ſmaller index A. (TAB. IV.) fig. 1. which moves in the circle *b*, *b*, *b*, *b*. about 4 inches diameter, is fixed on the axis of a double pulley B. fig. 2. nearly 1 inch diameter, which makes one revolution by a riſe, or fall of 3 inches of the mercury in the tube.

The pivots of the axis of the pulley B, are placed, each within 3 friction wheels, *e*, *e*, *e*, fig. 2. 2 inches diameter.

The larger index E. fig. 1. which moves in the circle *g*, *g*, *g*, *g*, — 10 inches diameter is placed on the axis of the double pulley H, about $\frac{1}{3}$ inch diameter, to make 3 revolutions exactly, for 1 revolution of the pulley B. Each of the pivots of the axis of this pulley is placed on 2 friction wheels *ii*, fig. 2.

The circle *b*, *b*, *b*, *b*, fig. 1. is divided into 3 equal parts; one for each inch the mercury riſes or falls in the tube, and is marked 28, to 31 inches, with rain, changeable, fair, &c. inſcribed, as other barometers generally are. Each of theſe parts is ſubdivided into 10.

The larger circle *g*, *g*, *g*, *g*, is divided into 300 equal parts, and marked 10, 20, &c.

The ivory cone, *d*, which reſts on the mercury in the tube, is ſuſpended by a ſilk thread *k*, fig. 2, which ſurrounds the outward pulley, B, and paſſes alſo 4, or 5 times round the outward pulley H, to which the end is faſtened.

The

The thread, *l*, by which the counterbalance, *dd*, is ſuſpended, ſurrounds in like manner the inward pulleys B, and H, but in a contrary direction. The weight *d* is about 8 grains heavier than the counterbalance, *dd*, which is fully ſufficient to move both the indexes, and alſo the regiſters; and is ſo ſlight a weight, as does not at all impede the riſing of the mercury in the tube.

As the cone *d* is ſupported by the mercury; when it riſes or falls in the tube, the pulleys B and H are moved round accordingly; and as a riſe or fall of 3 inches cauſes one revolution of the pulley B, and 3 revolutions of the pulley H, the indexes A, and E, placed on the axes of theſe pulleys, muſt be carried round the circles *b*, *b*, *b*, *b*, and *g*, *g*, *g*, *g*, in like manner.

The index A is moved $\frac{1}{3}$ of the circle *b*, *b*, *b*, *b*, by 1 inch riſe, or fall of the mercury, and the index E the whole circle *g g g g*; which, being about 30 inches in circumference, and divided into 300 equal parts, ſhews half a diviſion very perceptibly; ſo that the index E marks diſtinctly the 600th part of an inch riſe or fall of the mercury. And by placing the regiſters *f*, *f*, fig. 1. cloſe to the index, it muſt carry one along with it, when it moves; leaving it on its return, to mark the limits of its motion.

It requires a good deal of nicety and exactneſs in making and placing the friction wheels and pulleys; which require to be very light, and the pivots fine and ſmall. The machinery of this, as alſo of the former, was made by Mr. Vulliamy, watchmaker to her Majeſty; which is ſo well adjuſted, that it has not been in the leaſt out of order, in nine months I have had it; nor the other in nine years.

The exactneſs and facility with which an account of the variations in the weight of the atmoſphere may be kept, with the help of a barometer of this kind, muſt be very evident. I have frequently found, by extraordinary variations that have happened in the night, when the wind has riſen conſiderably, how little the obſervations made with common barometers are to be depended on; and have ſeveral times found by the regiſters, that the mercury had ſunk 50 or 60 diviſions; and one night particularly had ſunk 117 degrees, and returned within a degree and half of the place I had marked it on going to bed. When a ſtrong guſt of wind riſes, one may very plainly perceive the index of this latter barometer to ſink ſeveral diviſions, and riſe again as it abates.

Beſides the ſatisfaction that a barometer of this kind might afford to a curious obſerver, I ſhould imagine it might alſo be uſefully applied to the finding the height of the atmoſphere; with a much greater degree of exactneſs, at leaſt, than can well be afforded by any other.

It is generally allowed from experiments, that a column of air 72 feet high is equal in weight to 1 inch of water of the ſame baſe; ſo that, if the air were of equal denſity throughout, the atmoſphere could be little more than 5 miles high. But as the denſity is found to decreaſe by the difference of preſſure, and the air to be more rarified or expanded in proportion to its diſtance from the earth, it ſeems reaſonable to conclude, that if, by accurate experiments,

ments, the ratio of its decreaſe were found regular in proportion to the diſtance from the earth, its height might be eſtimated with a much greater degree of preciſion than it has been hitherto; though it ſeems generally allowed that its real height cannot poſſibly be aſcertained.

The impoſſibility of obſerving the difference of the preſſure of the atmoſphere at ſmall diſtances with accuracy by a common barometer, the ſcale of which is but 3 inches, is very evident; how far this inſtrument, the ſcale of which is 90 inches, might be conducive to the purpoſe, is ſubmitted to the judgment of others.

I ſhould imagine it would not be difficult, with a proper teakle, to raiſe a barometer of this kind gently, as high as 200 feet; and if it were raiſed from the ground, and let down again from each diſtance of 20 feet, the regiſters would mark very exactly to the 600 part of an inch at what height the mercury ſtood at each diſtance; ſo that the weight of each column of air of 20 feet, to the height it could be raiſed, would be found pretty exactly. And if a proper apparatus were fixed for raiſing the barometer, the experiments might be repeated, as often as requiſite, with very little trouble.

Experiments of this nature cannot well be made by a ſingle perſon; but if an inſtrument of the kind ſhould be deemed uſeful for the purpoſe, I ſhould very willingly lend it, and alſo any aſſiſtance I could give, in making the experiments.

I ſhall with great pleaſure ſhew you the inſtrument whenever you favour me with your company;

or to any of the gentlemen of the ſociety, who may think it worth examining.

I am, SIR,

Your moſt obedient ſervant,

Poland Street,
Nov. 8, 1769.

Keane Fitz Gerald.

XI. *Some Obſervations upon an inedited* Greek *Coin of* Philiſtis, *Queen of* Syracuſe, Malta, *and* Gozo, *who has been paſſed over in ſilence by all the ancient Writers. In a Letter to* Mathew Maty, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Cuſtos Archivorum of the Univerſity of* Oxford, *Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

I.

SIR,

Read March 1, 1770.

THE antient piece I propoſe to conſider (ſee TAB. V. n. 1.) here has a place aſſigned it in the very valuable (1) collection of the Reverend and learned Mr. Godwyn, Fellow of Balliol College, Oxford, who has been poſſeſſed of it ſeveral years. It exhibits on one ſide the ſame veiled head of a woman that occurs on a coin of Gozo, by me (2) heretofore deſcribed; and on the other the figures forming the type, or ſymbol, on the reverſe of that coin. Before the face of the veiled head, on Mr. Godwyn's piece, the Greek word ΒΑΣΙΛΙΣΣΑΣ preſents itſelf to our view; and on the reverſe the name ΦΙΛΙΣ-

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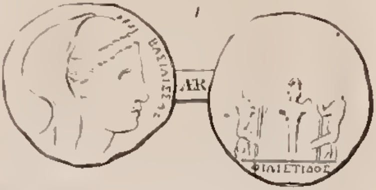
(1) This very valuable collection is now depoſited in the Bodleian Library, Oxon; Mr. Godwyn, who died in April, 1770, having bequeathed his noble library, and rich cabinet of antient coins, to the Univerſity of Oxford.

(2) *Philoſoph. Tranſact.* Vol. LVIII. p. 261, 262.

ΤΙΔΟΣ,

1



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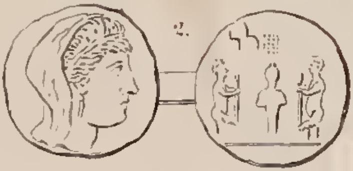
2.



Penes Joannem Swinton, S.T.B. Oxonienſ. R.S.S.

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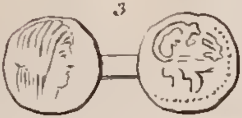

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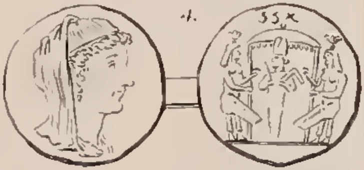
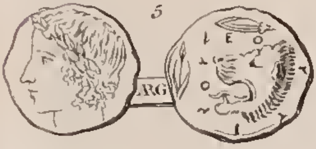


Spon, *Recherch. Curieus. d'Antiquit.* p. 462.

5



Apud Joannem Swinton, S.T.B. Oxonienſ. R.S.S.

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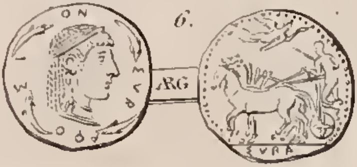

Apud Joannem Swinton, S.T.B. Oxonienſ. R.S.S.

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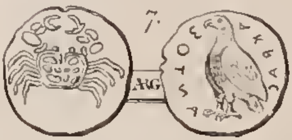

Apud Joannem Swinton, S.T.B. Oxonienſ. R.S.S.

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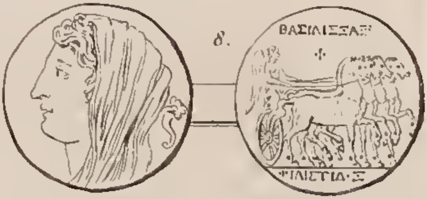

Apud Joannem Swinton, S.T.B. Oxonienſ. R.S.S.

Basire sc.

ΤΙΔΟΣ, PHILISTIDIS, in the exergue. The medal is of nearly the ſize of the middle Roman braſs, or rather of ſome of the Syracuſian braſs coins of the middle form. The head on the anterior part is tolerably well preſerved, but the type on the other is in much the ſame ſhattered condition as that exhibited by a coin of Gozo, conſidered by me in a (3) former (ſee Tab. V. n. 2.) paper, both of them having ſuffered not a little from the injuries of time. In fine, were it not for the legends, in different languages and characters, theſe two antient pieces would agree in all reſpects, and might be conſidered as duplicates of the ſame medal. However, the Greek word on the anterior part, and the Greek name on the reverſe, will ſufficiently, as I apprehend, announce the piece in queſtion an inedited coin.

II.

From the very valuable medal before me, in conjunction with thoſe of Gozo, publiſhed by the (4) Marquis Scipio Maffei, (5) Sig. Abate Venuti, and (6) the Royal (ſee Tab. V. n. 2, 3.) Society, and that of Malta to be met with in (7) (ſee Tab. V. n. 4.) M. Spon, it will moſt evidently follow, that they are all coins of Philiſtis; and that this princeſs was queen of Malta and Gozo, when thoſe iſlands were under the domination of the Greeks, and occupied by them and the Phœnicians. Which if we admit, it will farther from thence follow, that all thoſe pieces were ſtruck before the Carthaginians were poſſeſſed of Malta and Gozo. For the ſettlement of the Phœnicians in thoſe iſlands was undoubtedly prior to that of the (8) Greeks, and the Carthaginians (9) ſucceeded the latter in the occupation of them. The medals therefore of Gozo by me formerly named Punic, ſince the diſcovery of Mr. Godwyn's coin, I would rather denominate (10) Phœnician, as being ſtruck when the Phœnicians remained in that iſland. This ſeems to have been ſuſpected by (11) Sig. Abate Venuti, when he affirms the piece ſo perfectly ſimilar to Mr. Godwyn's to be a Phœnician medal, or at leaſt one of the moſt antient Carthaginian coins; but, by Mr. Godwyn's piece, it is rendered abſolutely inconteſtable. We may therefore conclude, that the princeſs whoſe head appears on all theſe medals was queen of Malta and Gozo, before the Carthaginians were ſettled in either of thoſe iſlands; though the time when ſhe ſwayed the ſcepter there cannot, for want of ſufficient light from antient hiſtory, with any preciſion, be ſo eaſily aſcertained.

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(3) *Philoſoph. Tranſ.* ubi ſup.

(4) Scip. Maff. *Veron. Illuſtrat.* Lib. iii. c. vii. p. 259. In Verona, 1732.

(5) Ridolfino Venut. *Diſſertaz. ſopra alcun. Medagl. Malteſ.* in *Saggi di Diſſertaz. Accademich. pubblic. let. nella uobil. Accadem. Etruſc. dell' antichiſſ. cit. di Cortona.* Tom. I. p. 36, 37. In Roma, 1735.

(6) *Philoſoph. Tranſact.* ubi ſup. p. 261, 262.

(7) Spon, *Recherch. Curieuſ. d'Antiquit.* p. 452. A Lyon, 1683.

(8) Thucydid. Lib. VII. Diod. Sic. Lib. V. Phil. Cluver. *Sicil. Antiqu.* Lib. II. p. 444. Lugd. Batavor. 1619. Ridolfin. Venut. ubi ſup. p. 35, 36.

(9) Thucyd. Diod. Sic. Cluver. & Venut. ubi ſup. Scyl. Caryand. *Peripl. Mar. Mediter.* Burchard. Niderſt. *Malt. Vet. & Nov.* p. 35. Helmeſtadii, 1660.

(10) That theſe pieces were not properly Punic, ſeems farther to appear from the types, or ſymbols, they exhibit; which are very different from thoſe, that generally preſent themſelves to our view, on the proper Carthaginian coins.

(11) Ridolfin. Venut. ubi ſup. p. 36.

III.

We meet with a (12) very minute Greek inſcription, conſiſting of only the two words ΒΑΣΙΛΙΣΣΑΣ ΦΙΛΙΣΤΙΔΩΣ, cut in the ſteps of the antient theater at Syracuſe, and (13) inſerted in the volume of the inſcriptions found in Sicily, juſt publiſhed at Palermo, by the Prince di Torremuzza; who from thence collects, not unjuſtly, as I apprehend, that Philiſtis was queen of Syracuſe. This had been before conjectured by the learned (14) Sig. Havercamp, but conjectured only; whereas the inſcription here mentioned renders it, at leaſt, extremely probable, and even, in conjunction with other conſiderations, almoſt, if not altogether, inconteſtable. I ſay, in conjunction with other conſiderations, as ſeveral reaſons have been aſſigned for placing queen Philiſtis on the throne of Syracuſe. 1. It has been obſerved, that the bigæ and quadrigæ, with the winged figure in them, which appear on Philiſtis's ſilver coins, ſeem much too grand for the ſovereign of only two ſuch petty and minute iſlands as Malta and Gozo. 2. The medals of Philiſtis, (15) adorned with the aforeſaid bigæ and quadrigæ, both in their type and workmanſhip, ſeem greatly to reſemble thoſe of Gelo, king of Syracuſe, and other Syracuſian coins.

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(12) Andr. Pigonat. *Stato preſente degli Antichi Monumenti Siciliani*, Tab. XVII. In Palermo, 1767.

(13) *Siciliæ & objacentium inſularum veterum inſcriptionum nova collectio, prolegomenis et notis, a Principe de Torremuzza, illuſtrat.* claſſ. vii. p. 61. Panormi, 1769.

(14) Sig. Havercamp. *Sicil. Numiſmatic.* Vol. II. p. 1014.

(15) We are told by Baron * Spanheim, who has been followed herein by M. † D'Orville, that none of the Syracuſian monarchs

* Ez. Spanhem. *De præſtant. & uſ. numiſmat. antiquor.* tom. I Diſſert. viii. p. 376, 545, 547. Edit. Lond. 1706.

† Jac. Phil. D'Orvil. *Sicul.* p. 457, 507. Amſtelodami, 1764.

before Hieronymus, the laſt of them, ever wore a diadem; from whence he concludes, that all the coins of thoſe monarchs, with diademated heads upon them, and particularly thoſe of Gelo, Hiero I. Dionyſius I. & II. Agathocles, and Hiero II. were poſterior to the times in which thoſe princes lived, and even to the reign of Hieronymus. But this falſe notion has been fully and effectually refuted by the Prince di [*] Torremuzza, in the prolegomena prefixed to his volume of antient inſcriptions found in Sicily, and printed at Palermo, this very year. The learned Prince firſt there ſhews, that Baron Spanheim is inconſiſtent with himſelf in this particular; and then to demonſtration proves, that the coins of Gelo, Hiero I. Dionyſius I. & II. Agathocles, and Hiero II. with diademated heads upon them, were actually ſtruck when thoſe princes ſat upon the throne of Syracuſe. To which I ſhall beg leave to add, that the elegance of the workmanſhip of the very antient Sicilian coins is no argument at all againſt their remote antiquity; as the Sicilians, and particularly the Syracuſians, who were the politeſt part of them, excelled in all the arts of peace, and therefore in the elaborate concinnity of their money, as well as military ſkill and bravery, in the days of Gelo, and even before, at leaſt, the latter part of that prince's reign. This has been clearly evinced by the learned Sig. [†] Bianconi, in the very valuable little treatiſe here referred to; in which, amongſt other things, he inconteſtably proves the medals of Gelo, and particularly thoſe mentioned here, to have firſt appeared in the reign of that prince, adducing at the ſame time ſeveral ancient Sicilian pieces even prior to thoſe coins. Nor will it be improper farther to remark, that what has been advanced by Sig. Bianconi on the latter head is confirmed by (ſee TAB. V. n. 5, 6, 7.) ſeveral medals of Leontium, or Leontini, Agrigentum, and Syracuſe, now in my ſmall cabinet; the forms of the characters on which moſt evidently demonſtrate

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[*] Princep. de Torremuz. *Siciliæ et objacentium inſular. veter. inſcription. nov. collect. prolegom. et not. illuſtrat. prolegom.* p. LIV, LV, LVI. Panormi, 1769.

[†] Joan. Baptiſt. Biancon. *De antiqu. litter. Hebræor. & Græcor. libel.* p. 37—45. Bononiæ, 1748.

3. The very (16) learned Father Frœlich takes the fabric of theſe pieces not a little to reſemble that of many of thoſe ſtruck in Sicily. I am therefore inclined to believe, that Philiſtis was queen of Syracuſe, at the ſame time that ſhe was ſovereign of Malta and Gozo; and that thoſe iſlands, whoſe Greek inhabitants were probably for the moſt part either Syracuſians or of Syracuſian extraction, were dependencies of Syracuſe, when the medals now in view firſt appeared. Nor can this, I think, after what has been ſaid here, be well denied.

IV.

As we find not the leaſt mention made of queen Philiſtis in antient hiſtory, and no certain chronological characters on her coins; I muſt not take upon me to aſcertain, with any degree of preciſion, either the end or commencement of her reign. It may nevertheleſs be preſumed, that ſhe (17) muſt have

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them to have preceded the middle and latter part of Gelo's reign. Other teſtimonies and arguments might be offered in ſupport of the point I am now upon, were it not altogether ſuperfluous to produce them here.

(16) Eraſm. Froelich. *Element. Numiſmatic.* p. 133, 134. Viennæ, Pragæ, et Tergeſti, 1758. Prin. de Torremuz. ubi ſup.

As the ſilver pieces here mentioned were antiently of a greater current value, and more remarkable, than any of the other coins of Philiſtis; they ſeem to have appertained to that ſpecies of money called PHILISTIDION by Heſychius. I have, (ſee TAB. V. n. 8.) one of them, perfectly well preſerved, of the tetradachmal form. We are told by Beger, that they have been mentioned by Suidas, but this is a miſtake. *Φιλιστίδιον, νόμισμά τι.* Heſych. Vide L. Beger. *Theſaur. Brand. Select.* Tom. I. p. 300, 301.

(17) That the reign of Philiſtis was anterior to that of Dionyſius I. will, I believe, from the following conſiderations, pretty clearly appear. 1. There ſeems to be a ſort of chaſm in the

preceded the elder Dionyſius, who aſcended the throne of Syracuſe about 404 years before the birth of CHRIST; which if we admit, it will perhaps be allowed probable, that the coins of Gozo

Syracuſian hiſtory for the interval between the expulsion of Thraſybulus and the beginning of the elder Dionyſius's reign, or rather for that part of this interval terminated by the aſpiration of Tyndarides to the regal dignity; or, at leaſt, the hiſtory of Syracuſe during this period muſt be allowed to be very imperfect, jejune, and indiſtinct. It is therefore more eaſy and natural to place queen Philiſtis there than in any part of the ſubſequent interval, between Dionyſius's acceſſion and the reduction of Syracuſe by the Romans: eſpecially, as the Syracuſian hiſtory during that period is full of great events, contains a pretty regular ſeries of the kings, tyrants, pretors, and rulers of that city; and conſequently will not, at leaſt with ſo much propriety, afford us room for a princeſs never mentioned therein. 2. Philiſtis's ſilver medals ſo much reſemble thoſe of Gelo, king of Syracuſe, as has been already obſerved, that we may from thence conclude her not to have been many years poſterior to that prince. 3. She was queen of Malta and Gozo, when the Phœnicians, in conjunction with the Greeks, occupied thoſe iſlands, and conſequently before they were poſſeſſed by the Carthaginians. But it ſeems extremely probable, from the teſtimony of Thucydides, that the Phœnicians ſtill remained there, when he wrote his hiſtory, and conſequently at the time of the Peloponneſian war. Nor do I find them mentioned, as poſſeſſed of Malta and Gozo, by any later writer. From whence we may conclude it not improbable, that the Carthaginians might have begun to ſettle there not many years after the end of that war; eſpecially, as they ſeem to have been poſſeſſed of thoſe iſlands ſome time, at leaſt, before Scylax of Caryanda, who, according to the very learned Mr. Dodwell, was cotemporary with Polybius, wrote his *Periplus Maris Mediterranei*, as we learn from that curious and valuable author. As therefore the firſt appearance of Philiſtis's Phœnician coins, ſtruck in the iſle of Gozo, muſt have preceded the earlieſt ſettlement of the Carthaginians there and in Malta, which probably happened not many years after the concluſion of the Peloponneſian war; and as queen Philiſtis's reign ſeems to have been ſucceeded by the exaltation of Tyndarides, which preceded, at leaſt, 20 years the commencement of that war; we may, I would preſume, now before me are more antient than any that have been hitherto tranſmitted down to us, with Phœnician characters upon them. For that the reign of Philiſtis was anterior to that of Dionyſius I. ſeems plain from hence, that there is no room for her in the ſubſequent part of the Syracuſian hiſtory, as I have juſt obſerved; the whole ſpace being filled with the reigns of princes and tyrants, of which we meet with a good account in antient hiſtory. Nor will it appear ſtrange, that what is advanced here ſhould be agreeable to truth, though hiſtory has been altogether ſilent as to the princeſs whoſe coins I am now endeavouring to throw ſome light upon. For we may infer from (18) Tully, that there were 27 kings and tyrants of Syracuſe, of which eleven have been unnoticed in hiſtory; and from (19) Valerius Maximus it ap-

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ſafely ſuppoſe that princeſs to have begun her reign above forty years before Dionyſius the elder aſcended the Syracuſian throne. Nor do I apprehend, that the learned, after having maturely weighed what has been here advanced, will refuſe their aſſent to ſuch a ſuppoſition. Diod. Sic. Lib. XI. p. 469, 470. Ed. Weſſeling. Amſtelodami, 1745. Thucydid. Lib. VII. Scyl. Caryand. Valer. Max. Phil. Cluver. Burchard. Niderſt. Ridolf. Venut. &c. *Univ. Hiſt.* vol. vii. p. 556. Lond. 1747.

(18) Cic. *Orat. in Verrem,* Lib. iv. cap. 55. Vide etiam. Prin. de Torremuzza, ubi ſup. p. 62. col. 2.

(19) Valer. Maxim. Lib. VI. cap. 2. extern. 2. Prin. de Torremuzza, ubi ſup. p. 62. col. 2.—The anecdote here referred to is as follows. "When the whole body of the Syracuſians "earneſtly wiſhed for Dionyſius's death, on account of the "extreme rigour and ſeverity of his government; a very old "woman offered up her prayers every morning to the gods for "the continuance of his life, and that ſhe might not ſurvive "him. The tyrant being informed of this, and wondering at "her unmerited benevolence, ſent for her, and aſked her how "he came to be ſo much in her favour? She replied in the fol- "lowing terms. "When I was a girl, we had a cruel tyrant, "whoſe death I greatly deſired. He was at laſt cut off by his

pears,

pears, that there muſt have been ſome intermediate princes between the elder Dionyſius and Gelo, kings of Syracuſe, whoſe names do not occur in any antient writer. We may therefore, as I apprehend, ſafely enough place queen Philiſtis in the interval between Dionyſius I. and Gelo, kings of Syracuſe, and even ſomewhere near the earlier of thoſe princes, as her ſilver medals ſo much reſemble Gelo's ſilver coins. If therefore it ſhould be ſuppoſed probable, that the pieces of Gozo, adorned with Phœnician letters, were ſtruck in that iſland, about 450 years before the commencement of the Chriſtian æra; the learned would not, I flatter myſelf, refuſe their aſſent to ſuch a ſuppoſition.

V.

When the Carthaginians firſt poſſeſſed themſelves of Malta and Gozo, as ſufficient light has not been afforded us by any antient writer in this matter, I cannot take upon me poſitively to ſay; but that they were ſettled there before the days of Scylax of Caryanda, who, according to the very learned Mr. Dodwell, was cotemporary with Polybius, we learn from that celebrated (20) author. We are informed moreover by

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" ſubjects, and ſucceeded by a greater tyrant; after whom you " mounted the throne. As therefore your government is more " intolerable than that of either of your wicked predeceſſors, I " am afraid your ſucceſſor will be ſtill worſe than you are, and " therefore I pray daily to the gods for your health and preſer- " vation." Hence it ſeems probable, that the interval between the expulſion of Thraſybulus, and the beginning of Dionyſius's reign, about 60 years, was filled up by the reigns of the two nameleſs tyrants here mentioned, and of ſome others who have been paſſed over in ſilence by the hiſtorians of Syracuſe. Valer. Maxim. & Prin. de Torremuzza, ubi ſup.

(20) Scyl. Caryand. in *Peripl. Mar. Mediter.* p. 50. Ed. Oxon. 1698.

(21) Thucydides, that in his days, or rather at the time of the Peloponneſian war, the Phœnicians, and not the Carthaginians, were inhabitants of Malta and Gozo. The Carthaginians muſt therefore have firſt occupied thoſe iſlands in ſome part of the interval between the Peloponneſian war and the age of Polybius; which is all we can, with ſafety, pronounce, (22) or decide, in this dark and intricate affair.

VI.

I have already offered it as my opinion, ſince an inſpection of Mr. Godwyn's medal, that the pieces of Gozo I (23) formerly attempted to explain are not Punic, but proper Phœnician, coins. Nor can it reaſonably be deemed an objection to this opinion that the maſculine plural termination of the inſcription is IN, and not IM. For that maſculine plurals, even in the days of Sanchoniatho, ſometimes, amongſt the Phœnicians, ended in IN, we learn from (24) Philo Byblius. The Phœnician words Beelſamen, or כעל שמין, and Zophaſemin, or צופה שמין, occurring in that author, to omit others that might, with equal facility, be adduced, and (25) particularly the legend צדנן, TZIDONIN, on the reverſe of a Sidonian

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(21) Thucydid. Lib. VII. Phil. Cluver. ubi ſup.

(22) That the Carthaginians, however, began to ſettle either in one or both of the iſlands here mentioned not many years after the concluſion of the Peloponneſian war, we may venture to pronounce probable, from what has been advanced in a preceding note.

(23) *Philoſoph. Tranſact.* Vol. LVIII. p. 235, 236, 261, 262.

(24) Sanchoniath. apud Euſeb. *Præp. Evang.* Vid. etiam Bochart. *Chan.* cap. II. p. 783. Francofurti ad Mœnum, 1681.

(25) Peller. *Recueil de Medailles de Peuples & de Villes, &c.* Tom. II. p. 221. Pl. LXXXII. n. 19, 20, 21, 22. A Paris, 1763.

coin,

coin, publiſhed by M. Pellerin, will ſet the point here inſiſted on beyond diſpute.

VII.

It may not be improper to remark here, that the pieces of Gozo conſidered in this and two of my former papers were (26), at firſt, taken for antient Malteſe medals, by M. Pellerin. Nor did I know, that he had altered his opinion, with regard to the place where theſe coins were ſtruck, till the month of June, 1769, when his laſt very valuable ſupplement ſufficiently apprized me (27) of that alteration. Had I known this ſooner, I would moſt certainly have mentioned it, either in one or both of my papers relative to theſe coins, inſerted in the laſt volume of the *Philoſophical Tranſactions*, which was then ready for publication. At ſuch a juncture as this, it was impoſſible for me to do that learned man juſtice, in this particular, there; which gave me, as it ought, no ſmall uneaſineſs and concern. And this I the more readily own, as I have the higheſt regard for M. Pellerin, on account of his exalted merit and abilities, as well as his ſingular modeſty, candour, and generoſity. His works moſt clearly demonſtrate his ſuperior learning, judgment, ſagacity, and penetration, at the ſame time that they diſcover not the leaſt tincture of that vanity and oſtentation which ſo ſtrongly mark the writings of certain other antiquaries, who yet make a very conſiderable figure in the learned world.

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(26) Peller. ubi ſup. p. 85, 86.

(27) Peller. *Quatrieme et dernier ſupplément*, &c. p. 57, 81. A Paris, 1767.

VIII.

VIII.

But though M. Pellerin and I both agree, in our notions of the place where the coins in queſtion firſt appeared, as well as our ideas of the Phœnician characters with which they are adorned; yet we differ conſiderably, in our lections of the word formed of thoſe characters. I read it CAVLIN, making it a gentile name, ſuch (28) as צדנן, TZIDONIN, צדנם, TZIDONIM, קסרנם, KOSVRNIM, &c. which we meet with on the medals of Sidon and Coſſura; this ſeeming to me the moſt natural lection, as well as the moſt conſonant to the manner and genius of ſuch minute inſcriptions on the proper Phœnician coins: whereas he reads this word COLL, or COLON; neither of which lections will, as I apprehend, prove ſatisfactory to the learned. For if we ſuppoſe both the ſecond and third letters to be *Lamed*, according to the eaſtern pronunciation (29), it ought to be read CALAL, or CALLAL, not COLL; but if we ſhould take the laſt letter for *Nun*, and pronounce the word COLON, the termination would ſeem to favour more of the Greek language than the Phœnician. Farther, if we ſhould chuſe the firſt reading given us by M. Pellerin, the correſpondent Greek word ought to be ΚΟΛΛΟΣ; if the latter, ΚΟΛΟΝΟΣ; neither of which words, as the old name of Gozo, unleſs I am greatly deceived, in any antient Greek writer, occurs.

IX.

Thus have I endeavoured to ſhew, that, by the aſſiſtance of a very curious and valuable inedited

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(28) Peller. *Supplém. aux ſix volumes, &c.* p. 27. A Paris, 1765. *Philoſoph. Tranſact.* Vol. LVIII. p. 237, 239.

(29) Val. Schindl. *Lex. Pentaglot.* p. 1614. Hanoviæ, 1621.

Greek coin, (30) ſome diſcoveries may be made; which will probably be looked upon as no inconſiderable acquiſition both to the ſcience of antient medals and antient hiſtory, by thoſe who are the moſt proper and competent judges of the affair. The communication of this, then, I would flatter myſelf, will by no means prove unacceptable to the Royal Society; who have always been ready to countenance and promote diſcoveries in (31) every branch of ſcience and

(30) I ſay ſome diſcoveries may be made; as the ſix following points, never hitherto diſcovered, ſeem clearly and naturally deducible from what has been advanced in this paper. 1. The coins of Gozo by me formerly explained, as well as all other ſimilar pieces, are to be reputed proper Phœnician, not Punic, coins. 2. They are Phœnician medals of queen Philiſtis, and were conſequently ſtruck in that princeſs's reign. 3. Philiſtis was probably queen of Syracuſe, as well as of Malta and Gozo. 4. The two iſlands laſt mentioned were, in all likelihood, dependencies of Syracuſe, when the coins in queſtion firſt appeared. 5. Philiſtis was anterior to Dionyſius I. king of Syracuſe, and ſeems to have lived about 450 years before the birth of CHRIST. 6. The Greeks and Phœnicians jointly occupied the iſlands of Malta and Gozo, during that princeſs's reign. Other curious particulars may probably hereafter tranſpire from Mr. Godwyn's moſt valuable inedited Greek coin, when it has been farther examined, and compared with other medals of Philiſtis, by the learned.

(31) To what is ſaid here, I muſt beg leave to add, that the Royal Society ſeem to have a peculiar right to this paper; as they did me the honour to publiſh two others, upon two Phœnician coins of Gozo, together with draughts of thoſe coins, in the laſt volume of the * *Philoſophical Tranſactions.* I formerly took theſe coins, and all others ſimilar to them, with † Sig. Abate Venuti, to belong to the iſle of Malta; having been led into that miſtake by his ‡ diſſertation upon them, and hindered from

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* *Philoſoph. Tranſact. vol.* LVIII. *p.* 235, ~~253~~ 261.
† *Ridolfin. Venut. ubi ſup.*
‡ *Id. ibid.*

uſeful literature. You will therefore indulge me the liberty of addreſſing this paper to you, in order to have it preſented to that very learned and illuſtrious body; and believe me to be, with great truth, and the moſt perfect eſteem,

Good Sir,

Your much obliged,

and very faithful,

humble ſervant,

Chriſt-Church, Oxon.
Nov. 27, 1769.

John Swinton.

diſcovering the place where they were ſtruck by the draughts of them given us by * Laſtanoſa, Paruta, and Sig. Abate Venuti, in which the ſecond and third characters are manifeſtly one and the ſame letter. But having received from the late Mr. Carmey, about twelve or fourteen years ſince, the two Phœnician medals of Gozo by me formerly explained, and publiſhed by the Royal † Society; I preſently diſcovered the ſecond letter on the reverſe of the firſt to be *Lamed*, and the third *Nun*. This induced me to attribute the piece to the city, and iſle, of Gozo; and having communicated my notion to three learned antiquaries, my particular friends, one of whom is ſtill living, very ſoon after the coin fell into my hands, they immediately declared themſelves of the ſame opinion.

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* *Venut. Laſtanoſa, Parut. ubi ſup.*

† *Philoſoph. Tranſact. ubi ſup. p.* 235, ~~255~~ 261.

XII. *A Letter*

Received November 28, 1769.

XII. *A Letter from Mr.* Thomas Woolcomb, *Surgeon, to the late Doctor* Huxham, *F. R. S. on the Caſe of a Boy, who died of a Gun-ſhot Wound, and communicated by the Rev.* J. Cor. Huxham, *A. M. F. R. S.*

SIR,

Read March 1, 1770.

I TAKE the liberty of troubling you with the following remarkable caſe in ſurgery, which, ſome months ſince, fell under my inſpection, as I judge it will not be wholly unacceptable to a perſon ſo eminent for his abilities in the medical art, and ſo diſtinguiſhed for a nice diſcernment into, and inveſtigation of, the more remote cauſes of morbid appearances; a few of which, that occurred in the preſent caſe, ſeem not a little unaccountable.

Dec. 17th, 1763, in the forenoon, I was ſent for to the aſſiſtance of John Kitt, a lad of about 15 years of age, who had juſt received a conſiderable wound by the unexpected going off of a gun loaded with ſmall ſhot, held near his arm. I found the ſhot, by being ſo near, had acted altogether as a ſlug, had lacerated much, and made a pretty large perforation through the *biceps* and *brachiæus internus* muſcles, had bared the *os humeri*, and in fine penetrated quite through the arm from below upwards.

By the time I arrived (which was almoſt immediately after the accident) I found little or no hæmorrhage, which made me hope the humeral artery had not been divided. On examining the wound, and finding no extraneous ſubſtances lodged, but the paſſage quite pervious to the probe; I dreſſed up with dry lint, digeſtive, &c. ordering moreover the whole limb to be wrapped up in a warm poultice made with oatmeal, ſtale beer, and a good deal of oil. Returning in the evening, I found the patient tolerably eaſy, but applying my fingers to the artery of the wriſt of the ſame hand, was not a little alarmed to find I could perceive not the leaſt pulſation. It was but too eaſy to apprehend the cauſe of it; that in all probability the artery was divided, and if ſo, the limb perhaps would not be ſaved.

I made my relation to the friends accordingly. However, as no threatening ſymptoms attended, I was willing to ſee, whether, if the artery was divided, the blood might not, as after the operation of the aneuriſm, find a paſſage by the collateral branches, and thereby the circulation be kept up. I was apt alſo to think, as there had been no hæmorrhage at the wound, that it might not be divided, but the courſe of the circulation be impeded only by ſome ſpaſmodic conſtriction, which poſſibly by the morning might relax and give way; at all events, I judged it moſt prudent to wait. I dreſſed up therefore with a little warm digeſtive, after proper fomenting the limb, and ordered the cataplaſm to be renewed as before. Little or no tenſion had yet taken place; nevertheleſs, in order to obviate that, and the ſymptomatic fever that might be expected, and finding withal the pulſe began to riſe, I ordered him to be bled about ℥xvj, and left him with a tourniquet put looſely round the arm, with proper directions to the attendants, for fear of any ſudden rupture of the blood-veſſel in the night.

The next morning I found him tolerably eaſy, but the pulſe very quick and ſtrong, and ſtill no pulſation in the wriſt of the wounded arm. The aſpect of the wound very good, no tenſion round. However, as it was ſo nice a point to determine, whether the artery was or was not divided, and of conſequence whether it would be more prudent, on the ſuppoſition it was, to proceed to amputation, or any longer run the riſk of a mortification's enſuing; I judged it proper to have other opinions, and for that purpoſe, called in three ſurgeons of credit in the town.

They were all of opinion, as there were no imminent ſymptoms, it was beſt ſtill to wait; judging rightly, that, if a mortification took place only through defect of the blood's circulation in the lower limb, it might eaſily be remedied by amputation above, time enough when it firſt made its appearance. We accordingly dreſſed up in the ſame manner, but had the patient bled again to ℥x or ℥xii, and gave a gentle lenitive, which procured a few ſtools. In the evening, ſymptoms were much the ſame; pulſe ſtill ſtrong and quick: bleeding was therefore repeated.

The next day every thing ſeemed to take a favourable turn, the pulſe grew much more calm, a good digeſtion came on, no tenſion at all was obſerved on the limb, and in this kindly manner we went on for three or four days. Though all this time not the leaſt pulſation could be felt on the wounded limb, there was always a kindly natural warmth on it, and the patient made no other complaint than of a numbneſs and deadneſs of his little and ring-finger.

By all theſe favourable circumſtances I was induced to hope all danger had now been over, when about the fifth or ſixth day from the accident, the appearance of the wound began to alter, and to look of a pale leucophlegmatic hue; the diſcharge became much more thin and ſerous, and very conſiderable *fungi* grew out from the ſurface of each wound; the whole limb both above and below the wound became greatly enlarged, the hand and fore-arm perfectly œdematous; the pulſe quick and ſmall, the countenance, from a freſh florid hue, ſunk, pale and ſallow.

Theſe alarming ſymptoms coming on gave me the greateſt reaſon to be apprehenſive of the event. To obviate them as much as poſſible, I ordered the *cortex* both in decoction and ſubſtance to be adminiſtered every hour or two, and had freſh conſultations with the other ſurgeons. It was not now practicable to amputate, as the diſtenſion of the limb extended quite to the *axilla.* We therefore continued the uſe of the *fotus,* warm dreſſings, &c. as before; ſtrewing over the *fungi* well with the *pulv. angel.* Notwithſtanding which, they continued to ſprout to a great height, and, though I pared away at every dreſſing all the dead ſurface with the knife, they baffled all endeavours to ſuppreſs them.

In this manner it continued to go on till the firſt of January, in the afternoon of which the man began to complain greatly of being cold; and, notwithſtanding the warmeſt and moſt invigorating medicines were given, he grew more and more ſo, till about eleven or twelve, when he expired. Neither before or after death was there the leaſt appearance of a mortification having taken place.

However, in order, if poſſible to inveſtigate the true cauſe of his death, and to ſatisfy ourſelves whether the artery was or was not divided, in the preſence of the other ſurgeons, I laid open the wounded parts, and paſſing a probe through the artery at a tranſverſe inciſion made above the wound, carefully diſſected away the ſurrounding integuments, and thereby diſcovered a perforation (about the bigneſs of a ſmall pea) made through the coats of one ſide of the artery.

We were all at a loſs to account, why there never enſued any hæmorrhage from ſo conſiderable a veſſel's being opened, as no eſchar could well have formed, nor yet appeared there any conſtriction or compreſſion; and yet it appeared as plain, that the courſe of the blood was thoroughly intercepted in that veſſel, by there never being the leaſt pulſation at the wriſt after the accident.

The cauſe of his death too at laſt ſeems to be pretty unaccountable, as no mortification enſued, which one would have expected to have been the natural conſequence of the blood's being ſo intercepted. If owing to the ſhock given the conſtitution, or *remora* to the circulation, ſhould not one have expected the ill conſequences would have been felt ſooner? whereas, for nearly the firſt week, no patient with ſo conſiderable a wound could go on better, no wound could have a better aſpect, or digeſt better.

By the repeated bleeding, lenient cathartics, and proper topical relaxing applications, all degree of tenſion was happily kept off, little or no ſymptomatic fever attended, and ſeemingly every ill ſymptom was obviated. In what manner then ſhall we conclude death at laſt to have been brought about ſo long after, ſince he neither ſunk under diſcharge from the wound, had no fever or convulſion, and no mortification ever appeared? and what ſhall we aſſign to be the true reaſon of no hæmorrhage enſuing, ſince there was ſo manifeſt an aperture through the coats of the artery? This I muſt confeſs myſelf wholly at a loſs to account for, and beg leave to ſubmit to your ſuperior judgment. I have laid before you a tedious indeed, but very exact and true ſtate of the caſe, and am, with great reſpect,

SIR,

Your moſt obedient

humble ſervant,

Plymouth, 25 June,
1764.

Tho. Woollcombe.

Received January 25, 1770.

XIII. *Journal of a Voyage, made by Order of the* Royal Society, *to* Churchill River, *on the North-weſt Coaſt of* Hudſon's Bay; *of Thirteen Months Reſidence in that Country; and of the Voyage back to* England; *in the Years* 1768 *and* 1769: *By* William Wales.

Read March 8 and 15, 1770.

⁂ It muſt be obſerved, that the Aſtronomical, and not the Nautical Day, is every where to be underſtood in the following Journal.

1768.

☉ May 29th. HAVING ſettled all my affairs in London; about 22 hours I ſet off for Greenwich, where I received my inſtructions from the Rev. Mr. Maſkelyne, his Majeſty's Royal Aſtronomer.

☽ the 30th. About 2 hours went on board a Graveſend boat; got to that place about 7, and went directly on board the ſhip. A. M. delivered my inſtructions to Mr. Dymond, for him to copy, according to Mr. Maſkelyne's directions.

♂ the 31ſt. About 2 hours weighed, and the wind being contrary, we tided it all the way from that place to Yarmouth road; where we arrived, and came to an anchor, about 20 h. on ♄, June the 4th.

We lay in Yarmouth Road until the 7th, when we unmoored, and came to an anchor in Cairſtown harbour on ☉, the 12th, about 14 hours; having had ſtrong gales, and thick weather, with drizzling rain almoſt all the time.

We lay in this place, and in the road, till ♃, the 23d, taking in ballaſt and live ſtock; having for the moſt part naſty thick, and cold fogs: About 16 hours the commodore made the ſignal to un-moor, and about 18 h. we got under way, and ſtood through Hoy-ſound. At 20 h. Hoy-head bore S. E. by compaſs, diſt. about 4 miles. At noon I obſerved the ſun's meridian altitude to be 54° 10′ ¼, whence the true lat. of the ſhip was 59° 3′ ¾; the courſe by compaſs ſince 20 h. was W. N. W. at the rate of 4 $\frac{6}{7}$ miles per hour. Hence the lat. of Hoy-head is 59° 2′ N. and if we account its long. weſt of Greenwich 3° 20′. the long. of the ſhip at this time was 4° 5′ W.

♀ the 24th. From the run and courſe of the ſhip, and different bearings therefrom, I deduced the latitudes of the following places, and their longitudes from the head of Hoy as annexed.

Skerry	lat.	59° 12′ ¼ N.	long.	0° 56′ ¼ W.	
Stack of Skerry		59 7 ¾ N.		1 3 W.	
Eaſt Barra		59 7 ½ N.		2 24 ½ W.	
Weſt Barra		59 5 N.		2 40 W.	

☿ June 29th. Being by account in long. 19° 40 W. and by a tolerable obſervation this day at noon in lat. 56° 57′ 20″ N. I took the following obſervations to aſcertain the time by Mr. Dymond's watch.

Time per watch.			Alt. ☉'s L L.	
4h	58′	14′	27°	55¼
5	2	55		21
	3	41		15¼
	4	27		9
	5	16		0¾

The quadrant was truly adjuſted, and the height of the eye above the water about 12 feet. Hence I infer that the watch was too faſt for apparent time 3′ 53″.

At 13h 5′ the eclipſe of the moon was conſiderably begun; I eſtimated it about 3 digits.

At 14h 11′ I judged the beginning of total darkneſs happened; but clouds rendered it a little uncertain

	14	11	0
Watch too faſt		3	53
Beginning of total darkneſs obſerved	14	7	7
Time at Greenwich by ſtrict calculation	15	13	50
Long. Weſt = 16 40¾	= 1	6	43

This muſt be conſiderably too little. Indeed I am of opinion my reckoning is ſo, as we have, theſe two laſt days, had a great ſwell from the eaſtward, which I have not ſkill enough in navigation to allow for.

♃ the 30th. By comparing Mr. Dymond's watch with Captain Richards's, which he ſays goes very exactly, I find that Mr. Dymond's has gained at the rate of 9′ ½ per day; and therefore 3′ 38″ muſt be taken from the time of the obſervation, more than I yeſterday allowed, which will make the long. of the ſhip at 5 h. yeſterday 17° 35′ ¼; but this is undoubtedly too ſmall, a circumſtance which I cannot account for.

♂ July 5th. Being by account in long. 45° ¼ W. and by obſervation in Lat. 57° 43′ N. I made the following obſervations for determining the long. of the ſhip. Mr. Dymond obſerved the altitudes of the ſun, and Capt. Richards thoſe of the moon.

Diſt. ☽ & ☉ neareſt limb.	Alt. of ☉ L. L.	Alt. of ☽ L. L.
99° 30	40 41	22 57
29	41 11	22 37
25¾	41 52	21 55
23¾	42 4	21 37
23	42 20	22 24

There muſt be ſubtracted from the diſtances 48″ for the error of the quadrant: the other quadrants were right; but 3′ muſt be ſubtracted for the dip of the horizon.

The long. of the ſhip according to theſe obſervations will be found 47° 47′ ¾ W.

I did not make uſe of the teleſcope when I made theſe obſervations, as its field is too ſmall to uſe when the ſhip has much motion, which was the caſe at this time.

We were certain that we were now well a-breaſt of cape Farewell; having the two preceding days paſſed ſeveral pieces of drift-wood. This is alſo ſtrongly corroborated by the preceding obſervations.

♃ the 6th. About 4 h. we paſſed another large piece of drift-wood, which was the laſt we ſaw.

☉ the 10th. About 22 h. the lat. of the ſhip being 57° 58′ N. and long. (reckoned from the obſervation of the 5th) 51° 20′ W. I made the following

ing obſervations for determining the variation of the needle.

Alt. ☉ L. L.	Azim
49° 51′	31 37
50 36	33 0

3′ 18″ muſt be ſubtracted from the ☉'s alt. for the dip of the horizon.

From the firſt obſervation the variation comes out 31° 33′, and from the ſecond 30° 2′; and the mean of both is 30° 47′ ½ weſterly.

♄ the 16th. The former part of theſe 24 hours we ran through ſeveral very ſtrong replings of the tide, which made us ſuſpect that we might be nearer the entrance of the Straights than our accounts ſhewed us to be; and therefore about 11 h. the whole fleet brought to, as the fog was exceeding thick. We now compared all our reckonings, and alſo with the commodore and brig's people, and found them all to agree very near: mine, which was about long. 61° W. being the headmoſt.

About 16 h. we ſaw the firſt iſle of ice; but it was at too great a diſtance for me to give any farther account of it.

☽ July the 18th. This day, and yeſterday, we have run through ſeveral very ſtrong riplings of tide; and have paſſed by many iſlands of ice; but their diſtance, and the thickneſs of the fog, rendered it impoſſible for me to give any account of them.

♂ the 19th. Paſſed within a cable's length of a very large iſland of ice, or rather frozen ſnow, for it appeared to me to be nothing elſe. It was about as high out of the water as our main-top, and was adorned both on its top and ſides with ſpires; and indented in the moſt romantic manner that can be imagined.

♃ the 21ſt. At noon the fog was ſo thick that we could ſcarce ſee a man on the forecaſtle, and hearing two guns at ſome diſtance (the ſignal for bringing to) we hauled our courſes. About half an hour afterwards, we fired a gun; but having no anſwer, we tacked, and ſet our courſes, fore, and mizen ſtays; apprehending that the ſignal at noon was to tack, and that we miſſed one gun.

All the afternoon we fired guns at ſhort intervals, but could hear no anſwer. At 7 there came on a ſtrong gale, which with the riplings we were continually running through, made a very rough ſea, the waves beating continually over the quarter deck: and if we add to theſe, the thickneſs of the fog, and the great number of ice iſlands we were amongſt, our ſituation muſt be allowed to have been truly dangerous: we weathered one iſland in the morning, but by about three or four times the ſhip's length, and though ſo very near, we could not ſee its top for the fog.

♄ July the 23d. About ½ paſt 2, we made the iſland of Reſolution. which forms the north ſhore, at the entrance of Hudſon's Straits, bearing from us N. W. b. W. It lies, by my account, in lat. 60° 29′ ½ N. and long. 65° 9′ W.

☉ the 24th. This afternoon, being in lat. 61° 55 ½ N. and long. (by account) 67° 46′, I made the following obſervations for determining the variation of the needle.

Alt. ⊙L.L.	Azimuth.		Alt. ⊙L.L.	Azimuth.		
29 50½	67 30	variat. 38° 44′	25 10	55 20	variat. 40° 52′	Dip 3′ 18″.
29 13	64 20		24 34	54 40		
28 58	63 35		24 8	52 30		
			23 50	51 45		

Hence the mean of both gives the variation 39° 48′ W.

About 10 there came along ſide of us a boat, with ſeveral Eſkimaux women, and two or three boys; but no men. They traded with the people ſome of their cloaths, and a few toys of their own making; ſuch as models of their bows, harpoons, &c. but I ſaw nothing elſe that they had to trade; nor had they any weapons, either of offence or defence, along with them. The boat is ſo well deſcribed and delineated in Crantz's hiſtory of Greenland, that it is entirely needleſs to attempt it here.

This morning we ſaw the other ſhips under the land, bearing from us N. N. E. and whom we joined about noon.

The north ſhore of theſe ſtraits ſeems to be a chain of broken iſlands, or rather, large, bare rocks; one riſing, as it were, in perſpective above another. But I cannot help obſerving, that from the accounts of authors, I expected to have found them entirely covered with ice and ſnow; whereas I found them entirely bare, except in ſome very deep vallies. I apprehend, however, that this had not long been the caſe, as the water every where kept continually tumbling down the rocks in prodigious torrents.

☽ July the 25th. This afternoon I told 32 iſlands of ice as I ſtood on the quarter-deck. This number is about double of what I have ever ſeen before, at one time.

The manner in which theſe iſlands are formed has been matter of much diſpute; and various, indeed, have been the conjectures of ſeemingly very learned authors concerning them. But I cannot, however, help thinking, that the cauſe and manner of their formation might very eaſily be made out from a few principles; and theſe facts, obvious to every one who viſits theſe Straits. But as this would lead me to controvert the opinions of ſeveral Gentlemen, who in all probability, are much more converſant in theſe things than I can pretend to be, I ſhall only here put down thoſe circumſtances; and leave it to every perſon to form his own judgment of the matter.

1ſt. The northern ſhore of theſe Straits, as it is uſually called, is one continued chain of ſmall iſlands; which form almoſt an infinite number of little bays, and inlets. 2d. The rocks which form the ſhores, are very high; and in moſt places almoſt perpendicular. 3d. The water is very deep cloſe to the ſhore, in moſt places 60 or 70, and in ſeveral 120 fathoms, and upwards. 4th. The tides riſe here ſometimes upwards of 6 fathoms, and ſet through theſe Straits with great velocity, and almoſt in all directions. 5th. There fall, during the winter ſeaſon, vaſt quantities of ſnow, and at this time the froſts are alſo very intenſe. 6th. The wind blows from the northern quarter, at leaſt, three quarters of the year. 7th. During the months of June and July, the vaſt quantities of ſnow, which lie expoſed to the ſun, melt very faſt, and the water running into thoſe bays and inlets, muſt cauſe a ſtrong current to ſet off from the ſhore. 8th. There are no ice mountains any where on thoſe ſhores. 9th. Theſe iſlands come not from Greenland; but are continually ſeen to ſet that way, until they get out of theſe Straits; after which their courſe becomes more ſoutherly. Laſtly, they are not ice, but ſnow frozen. Theſe circumſtances, which are atteſtable by too great a cloud of witneſſes to be diſputed, will, I apprehend, form ſufficient data, whereby to give a very ſatiſfactory anſwer to the problem in queſtion.

This day, as I was obſerving the ſun's meridional altitude, there came along ſide of us three Eſkimaux in their canoes, or, as they term them, Kiacks, but who had very little to trade, except toys. None of theſe had along with them any weapon that I ſaw, except a kind of dart, evidently conſtructed for ſea purpoſes, as it had a buoy fixed to it, made of a large bladder blown up.

The men have on their legs a pair of boots, made of ſeal ſkin, and ſoled with that of a ſea horſe; theſe come barely up to their knees; and above theſe they have breeches made of ſeal, or deer ſkin, much in the form of our ſeamens ſhort trouſers. The remaining part of their cloathing is all in one piece, much in the form of an Engliſh ſhift; only it comes but juſt below the waiſt-band of their breeches, and has a hood to it, like that of a woman's cloak, which ſerves inſtead of a cap. Over theſe they have a kind of foul-weather jacket, made of the ſame leather with the legs of their boots, which they faſten very tightly about their necks and wriſts; and when they are in their Kiacks (which alſo are extremely well deſcribed by Mr. Crantz) are likewiſe faſtened in ſuch a manner round the circular hole which admits the man's body, that not the leaſt drop of water can get into it, either from rain or the ſpray of the ſea.

The dreſs of the women differs not from that of the men, excepting that they have long tails to their waiſtcoats behind, which reach quite down to their heels; and their boots come up quite to their hips, which are there very wide, and made to ſtand off from their hips with a ſtrong bow of whalebone, for the convenience of putting their children in. I ſaw one woman with a child in each boot top.

As to their perſons, they ſeem to be low; but pretty broad built, and inclined to be fat: their hands remarkably ſmall; their faces very broad and flat; very little mouths, and their lips not remarkably thick; their noſes ſmall, and inclined to what is generally termed bottled; their eyes are black as jet, and their eye-lids ſo encumbered with fat, that they ſeem as if they opened them with difficulty; their hair is black, long, and ſtraight; and notwithſtanding that they ſeem encumbered with a ſuperfluity of fleſh, they are remarkably briſk and active; more eſpecially in the management of their Kiacks, which exceeds every thing of the kind that I have ſeen. All I can ſay with regard to their diſpoſition is, that if they really deſerve the character which authors have given of them, they are the moſt complete hypocrites that nature ever formed *.

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* It may not be amiſs to obſerve here, that I have had, whilſt at Churchill, an exceeding good opportunity of learning the diſpoſition of thoſe people; as there are ſeveral of them come

♂ July 26th. This evening I obſerved the moon's meridional altitude 2° 35′ alt. of the eye 12 feet.

A little before noon the following obſervations were made for determining the magnetical variation :

☉'s Alt. L. Limb.	Azimuth	
37 $2\frac{1}{4}$	75° 25′	from a mean of 3 obſervations by the chief mate.
39 $52\frac{1}{2}$	89 50	from a mean of 4 obſervations by myſelf.
41 3	2 42	from a mean of 5 obſervations by Mr. Dymond.

almoſt every year, by their own free will, to reſide at the factory ; and can with truth aver, that never people leſs deſerved the epithets of "treacherous, cruel, fawning, and ſuſpicious ;" the contrary of which is remarkably true in every particular. They are open, generous, and unſuſpecting ; addicted too much (it muſt be owned) to paſſion, and too apt to revenge what they think an injury, if an opportunity offers at that moment ; but are almoſt inſtantly cool, without requiring any acknowledgement on your part (which they account, ſhameful), and I verily believe, never remember the circumſtance afterwards. Mr. Ellis obſerves, "That they are apt to pilfer from ſtrangers, eaſily encouraged to a degree of boldneſs ; but as eaſily frightened." Now I cannot help thinking that he would have conveyed a much better idea of them if he had expreſſed himſelf thus : They are bold and enterprizing even to enthuſiaſm, whilſt there is a probability of ſucceſs crowning their endeavours ; but wiſe enough to deſiſt, when inevitable deſtruction ſtares them in the face.

Perhaps few people have a greater genius for arts, which ſhews itſelf in every one of their implements, but particularly in their boats, harpoons, darts, bows and ſnow-eyes, which laſt are moſt excellently contrived for preſerving the eyes from the effect of the ſnow in the ſpring. But a volume might be written on theſe ſubjects, and perhaps not unentertaining.

The height of the eye about 12 feet. Lat. 62° 12′ N. And by calculating theſe three ſets ſeparately, and taking a mean of the three reſults, the variation will come out $= 44^\circ \frac{3}{4}$.

It may perhaps be worthy of remark, that the iſland of God's Mercies, or, as ſome call it, the Upper Savage Iſland, lies in the mouth of an inlet, running northward; out of which come the greater part of thoſe iſlands of ice, which are ſo much taken notice of in theſe parts. I have been told by gentlemen in the Hudſon's Bay ſervice, that ſome of their ſhips have formerly been driven by the ice into this inlet, where they found a fine open ſea, without any bounds that they could ſee to the northward. This inlet is called the North-Bay.

I beg leave likewiſe to mention what I apprehend to be a miſtake in Crantz's hiſtory of Greenland, where he ſays that thoſe pieces of ice which are of a vitriol colour are ſalt, and conſiſt of ſalt water frozen to ice; but I can, from my own experience, aſſert, that when the ſalt water, which they catch by the ſea waſhing over them, is wiped clean off, they are entirely freſh. I will not take upon me to ſay that they are not made from ſalt water; but if they are, it muſt have depoſited all its ſalts before it was frozen to ice.

☿ July 27. This evening I told 58 iſlands of ice, all going directly acroſs the Straits from the mouth of the above-mentioned inlet, at the rate of ſeveral miles per hour.

From this one circumſtance, we have an irrefragable argument to prove the impoſſibility of Capt. Middleton's hypotheſis, relating to the very ſlow progreſſive motion of theſe iſlands, and the long time which, he thinks, they take up in diſſolving. For, admitting his hypotheſis to be true, and that there were no other iſlands of ice but what came out of this bay; not only Hudſon's Straits, but even all the adjacent ſea would in a very few years be ſo entirely choaked up with them, that it would be impoſſible to force a ſhip amongſt them, could a maſter of one be found ſo imprudent as to venture; which muſt be inevitable deſtruction. The truth is, their motion and diſſolution are apparently ſo very quick, that I am of opinion it muſt be a pretty large iſland which is not diſſolved in one ſummer. How Capt. Middleton could drop into ſuch a palpable miſtake, is very difficult to ſay: he moſt certainly had as great an opportunity of informing himſelf of the truth of what he wrote on this ſubject, as any perſon whatever; and in this caſe had not the leaſt inducement, whatever he might be thought to have in others, to ſpeak contrary to his knowledge.

♀ July 29th. At 15 h. we hauled the wind to the ſouthward, the ice being quite thick a-head of us. At 19 h. hauled the wind to the N. W. and ſtood through the ledge of ice, as, for aught that appeared to the contrary, it might reach quite to Cape Walſingham, which now bore S. W. It conſiſted of large pieces cloſe jambed together: in the place where we attempted to paſs through, it was not quite ſo cloſe. It is really very curious to ſee a ſhip working amongſt ice. Every man on board has his place aſſigned him; and the captain takes his in the moſt convenient one for ſeeing when the ſhip approaches very near the piece of ice which is directly a-head of her, which he has no ſooner announced, but the ſhip is moving in a quite contrary direction to what it was before, whereby it avoids ſtriking the piece of ice, or at leaſt, ſtriking of it with that force which it would otherwiſe have done. In this manner they turned the ſhip ſeveral times in a minute; the wind blowing a ſtrong gale all the time.

A little before noon, being in lat. 62° 48′ N. and Cape Walſingham bearing S W. by S. I made the following obſervations for determining the magnetical variations.

Alt. ☉ L. L.	Azimuth.	Variat. W.
33° 48′	63° 40′	37° 19′
34 6	68 37	
34 39	68 30	
34 54	70 30	39° 6′
35 6	72 10	
35 11	71 0	
35 18	72 30	39° 18′
— 22	70 32	
— 30	74 30	

The alt. of the eye was 12 feet, and the mean of theſe three reſults gives 38° 34′ for the variation required. But it may be neceſſary to remark, that the very great motion which the ſhip had at the time, renders the obſervations dubious.

♄ July 30th. This evening I ſtaid upon deck till after midnight, in hopes to have obſerved the ☽'s diſtance from a ſtar; but, after trying for near an hour, I was obliged to give it up, on account of the twilights, which are amazingly bright in theſe high latitudes. There is another great inconvenience which attends obſervations of this kind here, viz. a

red

red hazineſs round the horizon, to a conſiderable height, rendering the ſtars very dim; but at the ſame time large, ſomething like the nucleus of a comet. I have been diſappointed by one or other of theſe, two or three times before; but this is the more vexatious, as we are now amongſt many iſlands, headlands, &c. whoſe longitudes are entirely unknown, and on which account an obſervation would have been ſingularly uſeful.

From this time to ♀, Auguſt 5th, no circumſtance material happened; but on that day I got the following obſervations for determining the longitude of the ſhip.

Diſt. ☽ & ☉'s neareſt Limb.	Alt. ☉ L.L.	Alt. ☽'s Cent.	Adjuſt. Quad.
73° 36′	33° 47′	48° 0′	2½ +
34	34 9	47 50	2 +
32½	22	47 23	3 +
32	35	47 5	3 +
32	48	46 57	2½ +
32	35 2	46 55	5)13(=2 36+
31	14	46 40	
30	21	46 35	
29½	32	46 23	
29	40	46 20	

Lat. 60° 46′½ N.

Theſe obſervations give the longit. of the ſhip W. 92°.

⁂ N. B. The height of the eye above the water was 12 feet.

♄ Auguſt 6th. Mr. Dymond obſerved the following diſtances of the ſun and moon, which I have taken the liberty to tranſcribe, and calculate by Mr. Dunthorne's method.

☽ à ☉		Alt. ☉ L.L.		Alt. ☽ Cent.	
60	56½	26	9	55	2
	56		30		4
	55		42		4
	54	27	00		5
	54		12		5
	54		20		7
	52½		40		5

There are no adjuſtments to be allowed; the height of the eye above the water was twelve feet: and the longitude of the ſhip, hence deduced, is 94° 2′¾; latit. = 59° 28′ N.

The longit. of the ſhip at noon was, according to
my obſervation of the 5th, 93° 50′ } Weſt.
Mr. Dymond's of this day, 94 21 }

☉ Auguſt 7th. About 5 ſaw the low land of Cape Churchill, bearing from the S. to S. W. b. S. but the hazineſs of the horizon made the land put on a different appearance every 4′ or 5′. I cannot help taking notice of one circumſtance, as it appears to me a very remarkable one. Though we ſaw the land extreamly plain from off the quarter deck, and, as it were, lifted up in the haze, in the ſame manner as the ice had always done; yet the man at the maſt head declared he could ſee nothing of it. This appeared ſo extraordinary to me, that I went to the main-top-maſt-head myſelf to be ſatisfied of the truth thereof; and though I could ſee it very plain both before I went up, and after I came down, yet could I ſee nothing like the appearance of land when I was there. I had often admired the ſingular appearance of the ice in theſe parts, which I have ſeen lifted up 2° or 3° at a diſtance of 8 or 10 miles, although when we have come to it, we have found it ſcarcely higher than the ſurface of the water.

At 21 h. we fired a gun, and thought we heard one in anſwer to it; which, if true, muſt have been from the factory.

☽ Auguſt 8th. We ſaw the flag-ſtaff of the factory, with the colours on it, bearing S. W. by W. but loſt it again in the haze a few minutes afterwards. At 3 we ſaw the factory-land, and the flag-ſtaff very plain, S. W. b. W. At 4 made the appointed ſignal, which was properly anſwered; after which, we bore away directly for the mouth of the river, and at 5 anchored, there being little wind, and the ebb tide was running out very ſtrong. At this time Cape Merry bore S. W. and Eſkimaux Point N. W. b. W. from whence, and the run of the ſhip ſince noon, I infer that the latit. of the factory is 58° 59′ N. and by Mr. Dymond's obſervation in 95° 33′ W. or, according to mine, 95° 2′ W.

A little before noon we weighed, and worked up the river to the uſual place where the ſhip lies, where, about two the 9th, ſhe was ſafe moored.

☿ the 10th, we went on ſhore, for the firſt time. We were met on the beach by Captain Richards, who went with us up to the factory, and introduced us, in form, to the governor, Mr. Moſes Norton, who, as well as Mr. Fowler, the perſon who ſucceeded him, behaved to us with great civility, and kindneſs. After breakfaſt, the ſurgeon of the factory was ſo kind as to walk with us ſeveral miles, to ſhew us the country.

The ſoil, as far as we went, conſiſted entirely of high bare rocks, or looſe gravel : amongſt the latter, there ſhoots up, in the lower places, many dwarf willows, and birch ; in the higher ones ſome ſmall gooſberry buſhes ; but theſe do not grow upright as in England, but creep along the gravel like the bramble brier. I ſaw beſides theſe ſome ſtrawberries, many cranberries, and a few bilberries ; but none of theſe were yet ripe, except a few of the laſt. I likewiſe ſaw ſome few plants creeping amongſt the moſs ; but none that I knew, except the dandelion and ſmall yarrow.

I ſaw ſome wild ducks and curlews, but could handle none of them ; we ſhot a few birds, much about the ſize, colour, and make of a woodcock : theſe they call here ſtone-plover. I ſaw another bird, not much unlike a quail, which they call here the whale-bird, from its feeding on the offal of thoſe fiſh after the oil is boiled out of it. Beſides thoſe, I ſaw many, and great variety, of the gull, or ſea-mew kind ; and alſo of ſmall birds, like our linnets, larks, &c. But the moſt extraordinary bird that I have yet met with is (I know not for what reaſons) called a man-of-war, and feeds on the excrements of other birds ; its way of coming at its food is alſo a little extraordinary ; he purſues the bird which he pitches on for his ſupply, until fear makes it void what he wants, and ſo ſoon as this happens, he catches the morſel in his mouth ; after which he leaves that bird and purſues another.

I found here three very troubleſome inſects. The firſt is the moſchetto, too common in all parts of America, and too well known, to need deſcribing here. The ſecond is a very ſmall flie, called (I ſuppoſe on account of its ſmallneſs) the ſand-flie. Theſe in a hot calm day are intolerably troubleſome: there are continually millions of them about one's face and eyes, ſo that it is impoſſible either to ſpeak, breathe, or look, without having one's mouth, noſe, or eyes full of them. One comfortable circumſtance is, that the leaſt breath of wind diſperſes them in an inſtant. The third inſect is much like the large fleſh-flie in England; but, at leaſt three times as large: theſe, from what part ever they fix their teeth, are ſure to carry a piece away with them, an inſtance of which I have frequently ſeen and experienced.

Auguſt 11th, 12th, 13th, 15th, 16th, 17th, and 18th, we got on ſhore the obſervatory and inſtruments; but the people were all ſo buſy unloading the ſhip, and repairing the quay, craft, &c. that we could not begin to put any part of the obſervatory up.

♂ the 16th, I went with Mr. Fowler about ten miles up the country, which, as far as we went, was nothing but banks of looſe gravel, bare rocks, or marſhes, which are over-flowed by the ſpring tides, and do not get dry before they return, and overflow them again. Our errand was, to ſee if we could not find ſome land likely to produce corn; and in all that extent we did not find one acre, which, in my opinion, was likely to do it. In ſome of the marſhes the graſs is very long, and with much labour they cut and dry as much hay as keeps three horſes, two cows, a bull, and two or three goats, the whole winter. I ſaw many acres of land covered with fir-trees, ſome of which might be perhaps about 20 feet high: theſe grow chiefly on the borders of the marſh-lands, or, which is the ſame thing, round the ſkirts of the rockey parts. I ſaw no other wood, of any kind, that would bear the name of trees; but, except where the rocks are entirely bare, or where the ground is covered with water every tide, it is entirely covered with low buſh-wood, after we get a few miles from the factory. Theſe ſhrubs conſiſt of willows of many kinds, birch, juniper, gooſeberry, and black currants. I ſaw ſeveral plants, very different from any which I have ever ſeen in England; but am not botaniſt enough to claſs, or even give a tolerable deſcription of them*.

Auguſt the 19th and 20th. We laid the foundation of the obſervatory in its proper place and poſition, which was on the S. E. baſtion, the higher and lower obſervatories nearly N. N. E. and S. S. W. of each other reſpectively. This place and poſition, though inconvenient in ſome reſpects, were, in my opinion, the moſt eligible for our purpoſe. We alſo got up the ſides thereof, and fixed up a ſtiff plank of dry Engliſh oak to ſcrew the clock to; this plank was about 5 ½ feet out of the ground, 4 feet in it, 16 inches broad, and 4 ½ thick, and ſupported with ſpurs to make it ſteady. There was likewiſe placed at the foot of it, in the moſt ſolid manner poſſible, a ſtone of about a quarter of a ton weight, with a flat ſurface, to ſet the bottom of the clock-caſe on; ſo that the clock ſtood entirely independent of the obſervatory.

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* I have brought ſome of them home with me in the beſt manner I could; but imagine they are not worth notice, on account of my want of experience in drying them.

The 22d and 23d, the people were allowed to write to their friends in England, ſo I employed myſelf to the ſame purpoſe.

The 24th, 25th, 26th, and 27th. The carpenters were employed in making us bed-places, &c. having hitherto had no where to lie but on the floor.

The 29th, 30th, and 31ſt, were employed on the obſervatory; we got on the circular parts and roof of each. On the 31ſt the ſhip ſailed for England.

September 1ſt, 2d, 3d, 5th, 6th, and 7th. We were employed in finiſhing the obſervatory. On the 8th we ſet up the two clocks. This morning the ſnow was about two inches deep on the plains. The 9th, put up the ſtove in the obſervatory, the two thermometers, and repaired ſuch parts as had been broken in the carriage. ♄ the 10th, we filled the barometer, and put it up; we alſo took out the quadrant, which we found much tarniſhed, eſpecially the arc, and adjuſted it ready for obſervation.

☽ Sept. 12th. I found that the roof of the obſervatory would not permit us to take zenith diſtances of any ſtars on the arch of exceſs of the quadrant, without moving it farther to the ſouthward; and as I could not hit on any method of determining the error of the line of collimation, which to me appeared ſatisfactory, except by obſervations of ſtars near the zenith, I reſolved to take up the floor of the obſervatory, and remove the piles on which the quadrant ſtood farther ſouth; and which, with the aſſiſtance of the houſe carpenter, I effected on the 16th, ſo as anſwer our purpoſe completely.

From this time to Auguſt the 28th, 1769, I kept no journal, except of the weather; the original of which has been given in to the Royal Society; and which is, in reality, the only thing we have to keep a journal of here in the winter ſeaſon; and therefore, what I have farther to offer is in ſhort memorandums, which I made when the circumſtance mentioned occurred to me; but as they will ſcarcely appear intelligible, in that form, to any but myſelf, I ſhall endeavour to throw it into a ſort of hiſtorical account of the ſeaſons, and manner of living, in that part of the world.

We arrived at Churchill juſt in the height of what is called the ſmall bird ſeaſon, which conſiſts of young geeſe, ducks, curlews, plover, &c. This begins about the latter end of July, and laſts till the beginning of September, when the greater part of theſe birds leave that part of the country. The geeſe then begin to go faſt to the ſouthward, and continue to do ſo until the beginning of October. This is called the autumnal gooſe-ſeaſon, in which every perſon, both native and European, that can be ſpared, is employed; but they ſeldom kill more geeſe at this time than they can conſume freſh.

By the middle of October the ground is generally covered with ſnow. The partridges then begin to be very plentiful; and as ſoon as that happens, the hunters repair to ſuch places as they think moſt probable to meet with plenty of game in. The Engliſh generally go out in parties of three or four, taking with them their guns, a kettle, a few blankets, a buffalo, or beaver ſkin coverlid, and a covering for their tent; which is made of deers ſkins, dreſſed by the natives, and ſewed together, ſo as to make it of a proper form and ſize. In pitching their tents, they have an eye likewiſe to their own convenience with reſpect to ſhelter from the winds, and getting of fire-wood; which, it will eaſily be imagined, makes a conſiderable article here in the neceſſaries of life: I mean at this ſeaſon of the year.

Much about this time, likewiſe, we who ſtayed at the factory began to put on our winter rigging; the principle part of which was our toggy, made of beaver ſkins: in making of which, the perſon's ſhape, who is to wear it, is no farther conſulted, than that it may be wide enough, and ſo long that it may reach nearly to his feet. A pair of mittens and a cap, of the ſame, are all the extraordinary dreſs that are worn by thoſe who ſtay at the factory, unleſs we add a pair of ſpatter-daſhes, made of broad cloth, which we wear over our common ſtockings, and two or three pair of woollen ſocks, which we have on our feet. Thoſe who go out add to the fur part of their dreſs a beaver ſkin cap, which comes down, ſo as to cover their neck and ſhoulders, and alſo a neckcloth, or cravat made of a white fox's ſkin, or, which is much more complete, the tails of two of theſe animals ſewed together at the ſtump-ends, which are full as long and thick as thoſe of the Lincolnſhire weathers before they are ſhorn. Beſide theſe, they have ſhoes of ſoft-tanned mooſe ſkin, and a pair of ſnow-ſhoes about 4 feet, or 4 ½ feet long. Moſt of theſe articles of dreſs I was furniſhed with by the honourable Hudſon's Bay company; but my cheſt was broken open, after the ſhip came up the river, and every article, except the ſnow ſhoes, taken away by the officers of the cuſtoms. And though there was not one thing which was not an article of dreſs; and though a petition was preferred to the Commiſſioners, in favour of Mr. Dymond and myſelf, yet, for ſome reaſon or other, they could not be reſtored.

But, to return to Hudſon's Bay. November the 6th, the river, which is very rapid, and about a mile over at its mouth, was frozen faſt over from ſide to ſide, ſo that the people walked acroſs it to their tents: alſo the ſame morning, a half pint glaſs of Britiſh brandy was frozen ſolid in the obſervatory. Not a bird of any kind was now to be ſeen at the factory, except now and then a ſolitary crow, or a very ſmall bird about the ſize of a wren; but our hunters brought us home every week plenty of partridges and rabbets, and ſome hares; all of which are white in the winter ſeaſon; and the legs and claws of the partridges are covered with feathers, in the ſame manner as the other parts of their bodies. We now killed two or three hogs which captain Richards had been ſo kind to leave with the governor, which before they were well opened, and cut into joints, were froze like a piece of ice, ſo that we had nothing to do but hang them up in a place where they would remain in that ſtate, and uſe them when we thought proper. We uſed ſome of theſe, I believe. in the month of May, which were as ſweet as they were the moment they were killed, and much more tender and delicate. One thing however muſt be obſerved, that if you roaſt them on a ſpit, or cut them in any manner whilſt roaſting, all the gravy will run out immediately.

In the fore end of December, I went to one of the hunters tents, where I ſtayed near a week. When

I was there, I was told by one of the people, that they had a ſpring very near them, which was not yet frozen over, notwithſtanding the ſea was frozen up as far as we could ſee, and the ice in the river was 4 or 5 feet thick. I went to ſee it; but that morning the froſt had been ſo very intenſe, that it was frozen over about an inch thick; when we broke the ice, the water was ſo ſhallow, that we raiſed all the mud from the bottom; and yet other ſprings, that were at leaſt ſix times its depth, had been frozen quite dry ſeveral weeks.

In the month of January, 1769, the cold began to be extremely intenſe: even in our little cabbin, which was ſcarcely three yards ſquare, and in which we conſtantly kept a very large fire; it had ſuch an effect, that the little alarm clock would not go without an additional weight, and often not with that. The head of my bed-place, for want of knowing better, went againſt one of the outſide walls of the houſe; and notwithſtanding they were of ſtone, near three feet thick, and lined with inch boards, ſupported at leaſt three inches from the walls, my bedding was frozen to the boards every morning; and before the end of February, theſe boards were covered with ice almoſt half as thick as themſelves. Towards the latter end of January, when the cold was ſo very intenſe, I carried a half-pint of brandy, perfectly fluid, into the open air, and in leſs than two minutes it was as thick as treacle; in about five, it had a very ſtrong ice on the top; and I verily believe that in an hour's time it would have been nearly ſolid. About the beginning of December we began to uſe ſpirits of wine for the plumb-line of the quadrant, which would have been evaporated to about half the quantity in a fornight's time, the ſpirituous part ſhooting up the plumb-line, and ſides of the glaſs like coral; but perfectly white. What remained would then freeze, but not before. At the beginning of the winter I hung a ſmall vial with about a tea-ſpoonful of proof ſpirits of wine by the thermometer, on the outſide of the obſervatory, and when I had well corked it up, dropped ſome water on the cork, which was inſtantly frozen to ice, and thereby ſealed the vial, in a manner hermetically. This, though it hung all the winter, never froze; nor, that I could perceive, altered its fluidity in the leaſt.

It was now almoſt impoſſible to ſleep an hour together, more eſpecially on very cold nights, without being awakened by the cracking of the beams in the houſe, which were rent by the prodigious expanſive power of the froſt. It was very eaſy to miſtake them for the guns on the top of the houſe, which are three pounders. But thoſe are nothing to what we frequently hear from the rocks up the country, and along the coaſt; theſe often burſting with a report equal to that of many heavy artillery fired together, and the ſplinters are thrown to an amazing diſtance.

On Sunday, March 19th, it thawed in the ſun, for the firſt time, and on the 26th it thawed in reality. The yard of the factory was that day almoſt covered with water. After this, it continued to thaw every day about noon when the ſun was out; and by the 23d of April, the ground was in many places bare. On the 26th it rained very faſt, almoſt the whole night, which was the firſt rain we had after October the 3d, 1768. It was really ſurprizing next morning

ing to ſee what an alteration it had made in the appearance of the country. We had now alternately ſnow and rain, froſts and thaws, as in England; the graſs began to ſpring up very faſt in the bare places, and the gooſeberry buſhes to put out buds: in ſhort, we began to have ſome appearance of ſpring.

The latter end of April, the hunters began to come home from the partridge tents, in order to prepare for the ſpring gooſe ſeaſon, which is always expected to begin about that time; and is, in truth, the harveſt to this part of the world. They not only kill, ſo as to keep the whole factory in freſh geeſe for near a month, but to ſalt as many as afterwards make no inconſiderable part of the year's proviſion. There are various ſorts of the geeſe, as the grey-gooſe, the way-way, the brant, the dunter, and ſeveral more, which I cannot now recollect. The gander of the dunter kind is, in my opinion, one of the moſt beautiful feathered birds that I have ever ſeen, their colours being more bright and vivid than thoſe of the parrot, and far more various.

Toward the latter end of May, the country began to be really agreeable; the weather being neither too hot, nor ſo cold, but that one might walk any where without being troubled with any diſagreeable ſenſation; and the dandelion, having grown pretty luxuriant, made moſt excellent ſallad to our roaſt geeſe.

On June 16th, the ice of the river broke up, and went to ſea; we now ſet our nets, and caught great plenty of fine ſalmon; I have known upwards of 90 catched in one tide. We had beſide, fiſhermen up the river, who brought us down plenty of pyke, mathoy, and tittymeg; theſe two laſt being fiſh peculiar to this country, and both very good. But, in enumerating the fiſh, I muſt not omit the kepling, which comes about the middle of July. This fiſh is nearly of the ſize of a ſmelt, and has exactly the ſame ſmell; but its back is much darker, and it is not quite ſo thick as a ſmelt in proportion to its length, more eſpecially toward the head: according to my opinion, it exceeds, in point of delicacy, every other fiſh whatever, and is in ſuch plenty, that they are thrown up, and left on the ſhore by the ſurf of the ſea; but then it muſt be owned that this rarity can never be had above a fortnight in a year, and ſometimes not ſo long. This fiſh is well known on the banks of Newfoundland.

About the beginning of July we likewiſe got plenty of very fine radiſhes; and the tops of our turnips began to grow large enough to boil for greens to our beef and ſalt geeſe. Moreover, towards the middle, we had very fine lettuce, ſo that if the muſchettos had not paid us a viſit about the beginning of the month likewiſe, the two or three laſt months would have been extremely agreeable; but, taking altogether, I cannot help thinking that the winter is the more agreeable part of the year.

I ſhall here add ſuch remarks as I have been able to make, relative to the natural hiſtory of the country; its inhabitants, ſoil, air, produce, &c. And firſt with reſpect to the inhabitants: They are of a middle ſize, but rather tall than otherwiſe; very ſpare and thin: I never ſaw one, either man or woman, inclined to be fleſhy; of a copper colour, wide mouths, thick lipped, and have long, ſtraight, black hair; of which they are immoderately fond, and would not have it cut, except on the death of a friend, for any thing that you can give them: their eyes are black, and the moſt beautiful that I ever ſaw. The reſt of their features vary as thoſe of Europeans do. Their diſpoſition ſeems to be of the melancholic kind; good-natured, friendly, and hoſpitable to one another, and to the Europeans; and I believe the moſt honeſt creatures that are any were to be met with. They do not readily forget an injury; but will never revenge it when they are ſober. They have no laws whereby to regulate their conduct, except that of reaſon; which, in their ſober moments, they are ſeldom known to tranſgreſs. They converſe extremely well on ſubjects which they underſtand, and are remarkably clever in repartees; but ſeem to have very little genius for arts or ſcience. They lead an erratic life, living in tents, as all people muſt do, whoſe ſubſiſtence depends entirely on hunting.

They are not without ſome notion of religion, but it is a very limited one. They acknowledge two Beings; one the author of all good, the other of all evil. The former they call Ukkemah, which appellation they give alſo to their chiefs; and the latter they call Wittikah. They pay ſome ſort of adoration to both, though it is difficult to ſay what. Their opinion of the origin of mankind is, that Ukkemah made the firſt men and women out of the earth, three in number of each; that thoſe, whom we Europeans ſprang from, were made from a whiter earth than what their progenitors were, and that there was one pair of ſtill blacker earth than they. They have likewiſe an imperfect traditional account of the deluge; only they ſubſtitute a beaver for the dove.

With reſpect to the ſoil and its produce of the vegetable kind, I can add very little to what I ſaid on my firſt coming on ſhore. As to corn, I am well convinced, that about Churchill it will produce none, except oats: thoſe, from a trial which I have ſeen, I believe might be brought to ſome tolerable degree of perfection in time, and with proper culture. Its internal contents are, I believe, chiefly rocks; there are, however, many of them marble, and ſome very fine. I have alſo ſpecimens of copper, copper ore, mundic, ſpars, talk (different from the Muſcovite), and ſeveral pyrites; for the greater part of which, I am indebted to Mr. Jacobs, chief, and Mr. Hutchins, ſurgeon at York Fort; from whom I received many favours.

The air in this country is very ſeldom, if ever, clear for twenty-four hours together; but we were not ſo much troubled with fogs as I expected we ſhould be, from the accounts which I had read of the country, and from what we experienced in our voyage out: but in this point, as well as every other which reſpects the weather, the journal which we kept will, I preſume, be moſt ſatisfactory.

I have before mentioned the haze which is continually found near the horizon here. This, I apprehend, is the cauſe why the ſun's riſing is always preceded by two long ſtreams of red light, one on each ſide of him, and about 20° diſtant therefrom. Theſe riſe as the ſun riſes; and as they grow longer, begin to be inflected towards each other, till they meet directly over the ſun, juſt as he riſes, forming there a kind of parhelion, or mock-ſun. Theſe two ſtreams of light ſeem to have their ſource in two other parhelia, which riſe with the true ſun; and in the winter ſeaſon, when the ſun never riſes out of the above-mentioned haze, all three accompany him the whole day, and ſet with him, in the ſame manner that they roſe. I hâve, once or twice, ſeen a fourth parhelion directly under the true ſun; but this is not common.

The aurora-borealis, which has been repreſented as very extraordinary in thoſe parts, bears, in my opinion, no compariſon to what I have ſeen in the north parts of England. It is always of the ſame form here, and conſiſts of a narrow, ſteady ſtream of a pale ſtraw-coloured light, which riſes out of the horizon, about E. S. E. and extends itſelf through the zenith, and vaniſhes near the horizon, about the W. N. W. It has very ſeldom any motion at all; and when it has, it is only a ſmall tremulous one on the two borders.

I ſhall now reſume my journal. Monday, Auguſt 28, we took down the inſtruments, packed them up, and put them on board the ſhip, expecting to have ſailed the next day; but unforeſeen accidents detained the captain until ♄ September the 2d, when we took leave of the governor and officers of the factory, and came on board the ſhip. We were after this detained by contrary winds until the 7th; on which day, about 15^{h} we ſaw the comet, which was obſerved this year in England, in a right line between ζ Orionis and Procyon; and alſo in a right line with Aldebaran, and α Orionis; but below both. About 18^{h} we ſailed out of the river with a fine breeze from the weſt; and at noon I obſerved, with great care, the ſun's meridional altitude to be 36° 2′ ¼; from whence the latit. of the ſhip is 59° 12′ ¼ and that of the factory 58° 55′ ½.

The prodigious difference between the latitude of Churchill factory, as laid down from obſervations made by Hadley's quadrant, and that deduced from the obſervations made with our aſtronomical quadrant on ſhore, has often employed my moſt ſerious attention; but I cannot think on any probable cauſe for ſuch difference, unleſs it lie in the very great refractive power of the air in theſe parts. I have mentioned how the ice and land appear to be lifted up, when we ſtand on the ſhip's deck: and if the viſible horizon be lifted up in like manner, it muſt make its apparent diſtance from the ſun, or, which is the ſame thing, the ſun's apparent altitude leſs than it otherwiſe would be; and conſequently, the latitude greater than the truth; and alſo greater than it will be ſhewn by a land quadrant, which depends not on the horizon, agreeable to what we find it in the caſe before us *.

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* Having mentioned this circumſtance to the reverend Mr. Maſkelyne, it immediately occurred to him, that the longitude deduced from obſervations of the ☽'s diſtance from the ſun or a ſtar, would be conſiderably affected by this cauſe, as not only the altitudes of the ☉, from whence the time at the ſhip is found; but alſo the latitude of the ſhip, found by an obſervation of the ſun's meridional altitude, or otherwiſe, will conſpire to encreaſe the ſun's diſtance from the meridian, or angle at the pole.

I have therefore recomputed the longitude from my obſervation of the moon's diſtance from the ſun, taken Auguſt the 5th, 1768, on a ſuppoſition that the mean error in any altitude taken by Hadley's quadrant, ariſing from this cauſe, is 10 minutes; and find that, on ſuch a ſuppoſition, which it muſt be allowed appear to be extremely well founded, the longitude will be 11′ ¼.

♀ September the 8th. About $14^{h}\frac{1}{2}$ I ſaw the comet again, in a right line with Saturn, and β Canis minoris; and alſo in a right line with Capella, and γ Geminorum. Its tail paſſed directly over ε Orionis and might be traced about as far beyond it, as that ſtar was from the head of the comet,

☉ September the 10th. At 16^{h} the comet was in a right line with Caſtor and Procyon; and about 5° or 6° below the latter. This was the laſt time I ſaw it.

☿ September the 20th. Being in latit. 61° 13′ ½, and Cape Reſolution E. b. S. I made the following obſervations for determining the longit. of the ſhip.

Diſt. ☽ & ☉ near. limbs.	Alt. ☉'s lr. limb.	Alt. ☽'s l. limb.
105° 52′ ½	17° 39′	35° 42
50	17 50	30
49	17 58	28
47	18 6	12

N. B. 1 ½ muſt be ſubtracted from the ſun's alt. for the error of the quadrant, and 3 ½ from both the ☉ and ☽'s altitudes for the dip of the horizon; and then the longit. of the ſhip will come out 66° 10′ W. By account, it is 65° 30′ ½.

Before I quit this part of the world, I cannot help obſerving, that I have had abundant reaſon, in my voyage home through Hudſon's Straits, and the adjacent Seas, to reſt ſatisfied with having ventured my opinion in reſpect to the quick motion, or ſwift diſſolution, of the ice iſlands. For, ſince we left the Straits, we have not ſeen one; and though we were

leſs than what I found it at the time when I made the obſervation, and therefore the longitude of Churchill will in this caſe be only 94° 50′ ¾ W. And by making a ſimilar correction of 15′ to Mr. Dymond's obſervation of the 6th, it will give the longitude of Churchill 95° 18′ W.

becalmed, and much troubled with contrary winds, ſo that we lay beating from ſide to ſide about nine days in the Straits, yet we did not ſee twenty iſlands the whole time, and theſe none of them very large. Whereas, was captain Middleton's hypotheſis true, and they were ſome hundred of years diſſolving, and traveling into the latitude of 50°, they could not have got by this time quite out of Hudſon's Straits, much more out of the Straits of Davis.

We were, by my account, abreaſt of Cape Farewell on the 26th; about which time we were taken with very rough, and contrary winds, with which we were troubled almoſt all the way till we got abreaſt of Ireland.

♄ October the 7th, P. M. being by account in long. 18° 57′ W. or by my laſt obſervation in long. 19° 38′, and lat. 52° 10′ N. I had the following obſervations for determining the longit. of the ſhip.

Diſt. ☽ & ☉ neareſt L's.	Mr. Wood ☉'s L. L.	J. D. ☽'s Cent.
86° 6′	17° 2	10′ 39
8	16 40	10 57
8½	16 30	11 8
10	16 3	11 27
10	15 50	11 38

1′ muſt be ſubtracted from the diſtances, 3′ from the ☽'s alt.; but 0 from the ſun's, for the errors of the quadrants; and 3′ 34″ from the ☉'s alt. and 3′ 18″ from the ☽'s for the dip of the horizon. The long. from theſe obſervations comes out = 18° 59′½ W.

☽ October the 9th. About 20^{h} we ſtruck ſoundings in 87 fathom water: the bottom fine white ſand. The 10th at 21^{h} made the Scilly light-houſe from the maſt-head; and at noon it bore directly north, by true card; the ſhip being in long. 5° 40′ W. by my account, in 5° 42′ ½ by my laſt obſervation, and in 6° 21′ by the former.

☿ October the 11th, latit. 49° 45′ N. I obſerved the following diſtances of the ☽'s neareſt limb from α Aquilæ.

Time by the Watch.	Diſt. ☽ à α Aquilæ.	Alt. ☽'s center.
8h 54¾	40 55½	33 46
59	58	33 45
9 1½	59½	33 38
4¼	41 0	34

Subtract 48″ from the diſtances for the error of the quadrant; and 3½ from the ☽'s alt. for the dip of the horizon. The watch went, as truly as it could well be regulated, to mean time.

Time by the Watch.	Alt. ☉'s L. L.
20 50	20 4½
54	8
54¾	12
56	26½
56¼	28¾

The error of the quadrant in theſe obſervations is — 48′, the dip of the horizon 3′ 34″, and the lat. of the ſhip 49° 33′, 3: and the watch will be found too ſlow for apparent time by 2′ 56″. The long. of the ſhip when I took the altitudes for determining the time was by account 4° 34′, by my firſt obſervation 5° 15′, and by the ſecond 4 36 ½ W. and it will be found by this laſt 4° 55′ W.

At noon the Lizard light-houſes bore N. E. by N. diſt. by eſtimation about eight miles; and from hence I infer that its longitude weſt from Greenwich is, by account, 4° 27′. By my firſt obſervation 5° 8′, by the ſecond 4 29 ½, and by the laſt 4° 48′. The true longitude of this place as determined by Mr. Bradley's obſervations made there (vid. preface to the Nautical Almanack of 1771) is 5° 15′ W. and therefore the greateſt error that I have committed in theſe obſervations is 45′ ½, and the mean of the three differs no more than 26′ ½ from the truth; but I apprehend the greateſt error will be thought of very little conſequence in the practice of navigation.

I here cloſed my account of the ſhip's way; but the evening of the 12th proving very fine, and having an opportunity of obſerving the ☽'s diſtance from Aldebaran to the eaſt, and from α Aquilæ to the weſt of her; I took the following ones, as they may be of uſe, if compared together, to ſhew what degree of accuracy theſe obſervations will admit of.

Time by the Watch.	☽'s fartheſt L. à Aldeb.	Alt. of ☽ L. L.	Time by the Watch.	☽'s neareſt L. à α Aquil.	Alt. of ☽ L. L.
9^{h} 57′	80° 19′	39° 9′	10^{h} 13′	51° 57′	38 55
10 6¼	80 14½	7	10 19¾	52 0½	47
10 8¼	80 12	6	10 22¼	52 1	43

The latit. of the ſhip at this time was 49° 34′ N. The error of the quadrant with which the diſtances were taken was—1′, and the dip of the horizon 3 ½. The obſervations of Aldebaran give the longitude of the ſhip 4° 10′ W. and thoſe of α Aquilæ 3° 43′ ½; but I would not be underſtood to mean that either of thoſe is the long. of the ſhip, becauſe I had no opportunity of finding the error of the watch.

On the 13th the captain, finding that he gained nothing by beating in the Channel, took a pilot on board for Plymouth; and on the 14th, about one o'clock, we came to an anchor in Hamoze. On the 16th, finding the eaſterly winds ſtill likely to continue, Mr. Dymond and myſelf took places in the ſtage, and arrived in London on the 19th about 9 in the evening.

I flatter myſelf that no gentleman will think that I have laid the preceding remarks before this learned

Society, under a preſumption that they can, in any reſpect, merit their notice. There are many of them on ſubjects which I am but little acquainted with: theſe were made only for my own amuſement, and are now ſubmitted to the Royal Society, at their command, and under a thorough conviction that they will be read with thoſe candid allowances, which, I am well convinced, they ſtand much in need of. At the ſame time, I ſincerely declare, that it would give me the higheſt ſatisfaction if they ſhould be found to contain one uſeful hint, or be conducive of pleaſure to any perſon whatſoever.

I ſhall conclude the whole with the latitudes and longitudes of the following places, which are all that I had an opportunity of determining with any tolerable degree of accuracy: and theſe all depend, more or leſs, on the ſhip's run; ſome of them for conſiderable diſtances. But they are done with care, and all the accuracy that I was capable of. Thoſe that are marked thus (:) are leſs certain than the others.

Names of Places.	Latit. N.	Long. W.
Cape Reſolution,	61° 29′	65° 16
Button's Iſles,		65 20:
Saddle-back Iſland,	62 7	68 13:
Iſland of God's Mercies, or Upper Savage Iſland,	62 32½	70 48½
Cape Charles,	62 46½	74 15
Saliſbury Iſland,	63 29	76 47:
Cape Walſingham,	62 39	77 48
Cape Diggs,	62 41	78 50
North End of Manſell Iſland,	62 38½	80 33
Cape Pembroke,	62 57:	82 0:

XIV. *Obſer-*

Received November 23, 1769.

XIV. *Obſervations on the State of the Air, Winds, Weather, &c. made at* Prince of Wales's *Fort, on the North-Weſt Coaſt of* Hudſon's Bay, *in the Years* 1768 *and* 1769, *by* Joſeph Dymond *and* William Wales.

1768.	Ho.	Baro.	Therm. A	Therm. B	Winds	Weather, &c.
			+	+		
♄ Sept. 10, at	22	29 79	41½	41½	N. E.	Rainy, with a gentle breeze.
☉ 11	0	89	42½	41½	Ditto	Ditto.
	4	92	42½	41½	N. W.	Cloudy and cold, the wind as above.
	18	30 04	40	41	Ditto	Ditto.
	20	06	41	44	Ditto	Ditto.
☽ 12	0	07	49½	48	North	Very hazy, and almoſt calm.
	4	05	50	45	N. E.	Cloudy, with a gentle breeze.
	9	29 99	46½	42	S. E.	Ditto.
	20	29 69	43½	44	Ditto	Very rainy, with ſtrong wind.
♂ 13	0	62	46	46½	South	Dittto.
	4	52	45	44½	N. W.	Ditto.
	20	35	40	38	North	Ditto.
☿ 14	0	38	40½	45	N. W.	Cloudy, wind high.
	4	42	41	41	Ditto	Ditto.
	18	29 55	37½	38½	Weſt	Ditto.

1768.	Ho.	Baro.	Therm.		Winds.	Weather.
			A	B		
			+	+		
♃ Sept. 15, at	0	29 61	46	42½	N. W.	Thin clouds, the wind high.
	5	66	47½	45	Weſt.	Thin clouds, and the wind high.
	20	29 74	36½	32	N. W.	Froſty, with ſleet at intervals, the wind very high.
						N. B. I could juſt perceive that ſome water on the head of a caſk, which ſtood cloſe to the obſervatory, had a thin ſkin of ice on it.
♀ 16	0	29 80	41	37½	N. W.	Cloudy, with ſleet at intervals.
	20	29 80	34	34	North	Cloudy, the air froſty.
♄ 17	4	29 79	38½	35½	N. W.	Cloudy, and ſharp air.
						W. W.
	18	29 62	32	32	N. E.	Cloudy, with froſty air.
☉ 18	1	29 61	43	38	Ditto	Cloudy.
	19	29 66	33	33	North.	Cloudy, with ſleet and almoſt calm.
☽ 19	0	29 73	36	38	Ditto	Clouds and haze.
	3	29 77	41	39	Ditto	Clear.
	11	29 84	36	34	N. W.	Partly clear, the aurora borealis very bright.
	18	29 96	34	34	S. W.	Clear, with a freſh breeze.
	22	30 03	40	36	Ditto	Ditto.
♂ 20	3	30 07	49	45	Weſt	Ditto.
	19	30 06	38	38	Weſt	Cloudy.
☿ 21	0	30 09	45	49	S. W.	Clear, fine weather.
	2	30 09	49	56	S. W.	Ditto.
	9	30 09	50	46	Ditto	Cloudy.
	18	29 98	45	43	South	Clear.
♃ 22	0	29 93	57	60	S, W.	Ditto.
	2	29 93	58	63	Weſt	Ditto, with high winds.
	4	29 89	62½	65	S. W.	Ditto.

1768.	Ho.	Baro.	Therm. A	Therm. B	Winds	Weather.
			+	+		
♃ Sept. 22, at	19	29 84	49	47	Ditto	Moderate breeze, and hazy.
♀ 23	0	29 81	53	56	South.	Ditto, cloudy.
	2	29 77	57	58	Ditto	Cloudy, almoſt calm.
	19	29 65	50	50	Calm	The latter part of this night much thunder and lightning; about 18^h it began to rain very faſt at times.
♄ 24	2	29 67	53	54	S. E.	Thick and cloudy; little wind.
	6	29 68	51	50	Eaſt.	Cloudy, with a freſh breeze.
						J. D.
	20	29 79	46½	46	Ditto	Cloudy, with a briſk gale.
☉ 25	0	29 81	47	45	Ditto	Foggy.
	4	29 81	46	42½	N. E.	Ditto.
	21	29 81	42	42	Ditto	Ditto, briſk breeze.
☽ 26	0	83	43	44½	Ditto	Cloudy.
	4	84	44	41	Eaſt	Cloudy, with ſqualls of wind.
	20	79	40	40½	S. E.	Ditto.
♂ 27	0	77	47	48	Ditto	Hazy, with a ſtrong gale.
	4	70	48½	47	Ditto	Cloudy, the wind very high.
	12	68	42	41½	Ditto	Ditto.
	20	67	41	40	Ditto	Ditto.
☿ 28	0	68	44	46	Ditto	Ditto.
	4	75	44	46	Ditto	Cloudy, but leſs wind.
	20	96	43½	44	South	Hazy.
♃ 29	0	98	50		Ditto	Clear.
	4	98	56	55	Ditto	Ditto.
	20	97	40	38	Ditto	Cloudy, freſh breeze.
♀ 30	0	29 98	47	47	Ditto	Flying clouds, ditto breeze.
	4	29 98	53	50	S. E.	Clear.

1768.	Ho.	Baro.	Therm.		Winds.	Weather, &c.
			A	B		
			+	+		
♀ Sept. 30, at	20	29 82	36	35	S. E.	Clear, and a ſharp hoar froſt.
♄ Oct. 1	0	29 79	46	45½	South	Clear ſharp air.
	4	29 72	49½	50½	Ditto	Flying clouds.
						W. W.
	19	29 34	40	39	Ditto	Clear.
☉ 2	0	29 25	49	50½	Ditto	Hazy.
	21	29 39	37	38	N. W.	Cloudy, and moiſt air.
☽ 3	1	29 47	39	36	Ditto	Ditto.
	5	29 50	38	34	Ditto	Snow and rain.
	19	29 65	31	29	Ditto	Cloudy, and very cold to ſenſation.
♂ 4	0	29 70	31	30	Ditto	Ditto.
	5	29 61	30	30	Ditto	Ditto.
	19	29 56	28½	29	South	Clearing, a hard froſt.
☿ 5	0	29 44	34	40	Ditto	Cloudy and warm.
	5	29 44	40	44	S. W.	Clear.
	14	29 42	38	33	N. W.	Clear; but the Aurora Borealis ſo bright in the zenith, that I could not ſee α Perſei.
	20	29 43	35	38	N. W.	Cloudy, wind high.
♃ 6	2	29 54	36	35	Ditto	Ditto.
	5	29 57	35	34	Weſt	Strong wind and ſleet.
	19	29 62	29	29	N. W.	Strong wind, a hard froſt, and cloudy.
♀ 7	0	29 62	30½	32	Weſt	Cloudy and high wind.
	5	29 63	31	33	Ditto	Ditto.
	19	29 73	30	28	N. W.	Strong wind, ſmall hail.
♄ 8	2	29 72	31	30	Ditto	Strong wind, cloudy.
	6	29 71	32	33	Ditto	Ditto, and clear.
						J. D.
	20	29 57	30	30	Weſt.	Sharp froſt and hazy, wind pretty high.
☉ 9	0	29 52	33	35	Ditto	Ditto, and cloudy.
	4	29 49	35	37		Ditto, and calm.
	20	29 48	34	34	N. W.	A ſtiff gale, and much ſnow falling.
☽ 10	0	29 50	35	35	North	Wind high, much ſleet.

1764.	Ho.	Baro.	Therm. A +	B +	Winds.	Weather, &c.
☽ Oct. 10.	4	29 53	35	34	N.W.	Ditto and ditto, a very keen froſt air.
	20	29 82	28	27	Ditto	Ditto. N. B. The liquid in which the plumb-line of the quadrant is immerſed, conſiſting of water, and about one fourth part brandy, is this morning froze ſo hard, that I can ſcarcely make an impreſſion on it with my finger.
♂ 11	0	29 88	27	25	N. W.	Cloudy, the wind high, and much frozen ſleet.
	4	29 94	27	26	Ditto	Leſs wind.
	20	29 85	23½	23	Eaſt	Cloudy, and the air extremely ſharp.
☿ 12	0	29 70	26	32	Ditto	Ditto, and a briſk gale.
	4	29 57	29½	31½	Ditto	Ditto.
	20	29 72	30½	33	N. E.	Cloudy, with ſome ſnow, and very little wind.
♃ 13	0	29 79	32	34	Ditto	Ditto.
	6	29 82	32	28	North	Clear, and very ſharp.
	20	29 85	28	26	Ditto	Ditto.
♀ 14	0	29 90	37	34	Eaſt	Ditto.
	4	29 90	41	37	Ditto	Ditto.
	20	29 87	30	32	South	Very warm, thick and foggy.
♄ 15	0	29 89	33	36	Weſt	Ditto.
	4	29 91	34	31	Ditto	Ditto.
						W. W.
	20	30 19	22	17	NNW	Very ſharp, with ſnow.
⊙ 16	0	30 20	25	28½	N. W.	Clear.
	4	30 19	27	21	Ditto	Ditto.
	13	30 17	22	20	Weſt	Coming on cloudy.

⊙ Oct.

1768.	Ho.	Baro.	Therm. A	B	Winds.	Weather, &c.
			+	+		
☉ Oct. 16, at	20	30 18	20	19	Weſt	Clear.
☽ 17	0	30 22	29	23½	Ditto	Ditto.
	3	30 20	31	28	Ditto	Ditto.
	15	30 04	26	23	S. W.	Ditto.
	20	29 93	26	27	South	Cloud to the eaſtward.
♂ 18	2	29 84	31	33	S. W.	Cloudy.
	5	29 81	31	32	Weſt	Ditto.
	20	29 77	32	32	N. W.	Ditto with ſleet.
☿ 19	0	29 79	32½	34	Ditto	Ditto, moderate wind.
	4	29 83	33	31½	Ditto	Ditto.
	20	29 81	30	30	Weſt	Cloudy, with ſome ſnow, and almoſt calm.
♃ 20	2	29 77	33	31		Cloudy and calm.
	20	29 55	32	34	S. W.	Cloudy, with a moiſt air.
♀ 21	0	29 55	33	36	Ditto	Ditto.
	5	29 55	34	36	Weſt	Ditto.
	21	29 64	31	27	North	A briſk fall of ſnow.
♄ 22	0	29 65	30	27	Ditto	Fair, but cloudy.
						J. D.
	20	29 54	23	25	N. W.	Wind very high, air ſharp, much ſnow.
☉ 23	0	29 54	25	26	Ditto	Wind high, and dark weather.
	4	53	26	26	Ditto	Ditto, with ſleet.
	20	58	23	18½	Ditto	Ditto.
☽ 24	0	65	21½	20	Ditto	Ditto.
	4	69	21	21	Ditto	Ditto.
	20	76	16½	13½	North	Ditto.
♂ 25	0	76	16	19	Ditto	Ditto.
	4	76	18	19½	Ditto	Ditto.
	20	78	15	9½	N. W.	Leſs wind.
☿ 26	0	71	17	9½	Ditto	Hazy.
	4	63	18	14½	Ditto	Very hazy.
	20	25	22	24	Calm	Quite thick and cloſe, with ſome ſnow.
♃ 27	0	23	24	28	N. W.	Briſk gale, and cloudy.
	4	22	25	27	Calm	Cloudy with ſleet.

1768.	Ho.	Baro.	Therm. A	Therm. B	Winds.	Weather, &c.
			+	+		
♃ Oct. 27, at	20	76	23½	14	North	Cloudy, wind high,
♀ 28	0	81	20	15½	N. W.	Hazy, wind very high.
	4	29 86	18	11	Ditto	Cloudy, wind ditto.
	20	30 01	10	10	Ditto	Dark and cloudy, wind as above. This morning a glaſs of brandy, expoſed in the open air, was frozen to the ſides of the glaſs.
♄ 29	0	30 05	12½	17	Ditto	Dark and cloudy.
	4	30 06	12	11½	Ditto	Clear.
						W. W.
	21	30 03	12	15	S. W.	Cloudy, little wind.
☉ 30	1	30 02	16	22	Calm	Cloudy.
	8	30 00	18	18	S. E.	Ditto.
	20	30 13	19	20	N. W.	Ditto.
☽ 31	0	30 16	20	22	Ditto	Ditto.
	5				N. E.	Ditto.
	20	30 21	13	14	S. E.	Clear.
♂ Nov. 1.	0	30 17	19	19½	S. E.	Very hazy.
	8	29 90	20	22	South	A briſk fall of ſnow.
	21	29 85	21	19	N. W.	Snowing.
☿ 2	0	29 85	20	19	Ditto	Ditto.
	8	29 67	16	10	S. W.	Cloudy.
	20	29 01	22	27	South	Snowing.
♃ 3	0	28 95	24	32	N. E.	Cloudy.
	8	28 99	24½	24	N. W.	Clearing.
	20	29 17	24	18	Ditto	Cloudy, ſtrong wind.
♀ 4	0	29 21	20½	16	N. E.	Strong wind and drifting ſnow.
	8	29 21	15	10	N. W.	Ditto.
	20	29 16	13	12	Ditto	Ditto.
♄ 5	0	29 18	14	16	Ditto	Ditto.
	9	29 34	13	9½	Ditto	Ditto.
						J. D.
	20	29 61	8½	0	Ditto	Hazy, wind very high.
☉ 6	0	29 64	7½	5	Ditto	Ditto, and much drift.
	4	29 65	7	2−	Ditto	Ditto.

☉ 6 Nov.

1768.	Ho.	Baro.	Thermome. A	Thermome. B	Winds.	Weather, &c.
☉ Nov. 6, at	18	29 56	−3	−10½		Clear and calm. A half-pint tumbler of Britiſh brandy was this morning frozen ſolid in the obſervatory.
	21	54	−4	− 7½	N. W.	Clear. The river frozen over, ſo that the people, this day, walked acroſs it.
☽ 7	0	50	0	0	Ditto	Hazy and calm.
	4	49	+ 3	+ 4	Ditto	Cloudy.
	20	19	+11	+18	N. E.	Thick, and ſnows very faſt.
♂ 8	0	29 33	+16	+30½	Eaſt	Ditto.
	4	54	+19	+20	N. E.	Ditto, the wind very high, and much drift.
	20	29 92	− 4	−12	N. W.	Very clear, the wind moderate.
☿ 9	0	29 91	− 1½	− 3	N.W.	Clear, and little wind.
	4	29 89	0	+ 3½	Ditto	Ditto.
	20	29 85	+ 3½	+ 4	Ditto	Dark thick weather.
♃ 10	0	29 84	+ 4½	+ 9½	Ditto	Ditto.
	4	29 83	+ 4	+ 8	Ditto	Ditto, ſome ſnow.
	21	29 82	+ 2	+ 2	Ditto	Very dark and thick.
♀ 11	0	29 77	+ 4	+ 9	Ditto	Ditto.
	4	29 75	+ 6	+ 8	North	Cloudy.
	20	29 48	+ 5	+ 6	S. E.	Cloudy, a briſk wind.
♄ 12	0	29 25	+ 7	+15½	Ditto	Ditto, with ſleet.
	4	29 19	+ 9½	+12½	Ditto	Cloudy.
	11	29 7	+15	+18	Ditto	Ditto.
						W. W.
	20	28 96	+20	+23	Eaſt	Snowing briſkly.
	22	Ditto	+22	+30	Calm	Snowing; at this very

time, * the Wind ſprung up at N. W. on which there was inſtantly a very ſenſible change in the heat of the air, ſo as to be much colder.

☉ Nov.

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* The remarks which I made on theſe phænomena are the following: It had been almoſt calm all this morning; but about 11 A.M. there aroſe, almoſt inſtan-

			Thermo.			
1768.	H.	Baro.	A	B	Winds.	Weather, &c.
☉ Nov. 13,	1	28 94	+20	+20	N. W.	Snowing and blowing ſtrong.
	8	29 15	+12	− 5	North	Ditto.
	*$9\frac{1}{2}$					†
	19	29 40	− 3	−12	N. W.	Very hazy.
☽ 14	0	29 46	− 4	− 6	Weſt	Very hazy ſouthward.
	4			− 9	Weſt	Hazy with moderate wind.
	8	29 53	− 4	− 6	WSW.	Cloudy with ditto.
	20	29 62	+ 4	− 4	Weſt.	Clear and moderate wind.
♂ 15	0	29 63	+ 1	+ 3	Ditto	Very hazy and ditto.
	8	29 66	+ 4	+ 6	Weſt	Cloudy.
	21	29 59	+ 1	− 5	South	Clear.
☿ 16	0	56	+ 2	+ 6	S. W.	Very clear.
	8	59	+ 9	+ 3	N. W.	Cloudy, with ſleet.
	11	62	+ $7\frac{1}{2}$	− 2	Ditto	Clear, the Aurora Borealis very bright.
	22	61	− 2	− 2	Ditto	Cloudy.
♃ 17	0	65	− 1	+ 3	Ditto	Ditto.
	8	73	− 1	− 9	Ditto	Clear.
	18	83	− 7	−12	Ditto	Ditto.
	22	84	− 9	−12	Weſt	Ditto.
♀ 18	0	87	− $6\frac{1}{2}$	− 7	Ditto	Thin clouds.
	8	88	− 8	−14	Ditto	Coming on hazy.
	19	90	− 3	−15	N. W.	Clear.
♄ 19	0	88	− 5	−$11\frac{1}{2}$	Ditto	Foggy.
	9	85	− 9	− 6	Ditto	Clearing.

inſtantaneouſly, a perfect hurricane in the north-weſt quarter, which brought along with it a prodigious quantity of ſnow, and alſo a very remarkable change in the temperature of the air.

* About half paſt 9 p. m. being juſt going to ſleep, I was diſturbed by a noiſe which I at firſt miſtook for the report of one of the guns (three pounders) which are on the top of the houſe; but which, I have ſince been informed by the officers of the factory, was cauſed by one of the beams of the houſe being rent by the expanſive power of the froſt. W. W.

† Heard a very uncommon ſound, ſeeming to be within the factory; and near as I can liken it, it was the ſame as firing a ſmall carriage gun; the factory people ſay it is very frequent in cold weather, and they imagine it to be the beams of the houſe, as the ſame is heard on board the ſhip.

N. B. On ☽ 14th, at 4 h. I, for the firſt time, had a fire in the obſervatory. J. D.

1768.	Ho.	Baro.	Thermome.		Winds.	Weather, &c.
			A. °	B. °		
♄ Nov. 19, at	12	85	−10	−15	N. W.	Coming thick.
						J. D.
	21	29 86	− 7½	− 4	Eaſt	Cloudy, little wind.
☉ 20	0	29 87	− 5	+ 1	Ditto	A little foggy, the wind as above.
	8	29 89	− 0	+ 2	N. E.	A briſk gale, with ſnow.
	20	29 87	+ 4	+ 5	North.	Ditto.
☽ 21	0	90	+ 5½	+10	Ditto	Ditto.
	4	91	+ 6	+ 7	Ditto	Ditto.
	20	30 13	+ 6	+ 3	N. W.	Wind high, very thick and dark.
♂ 22	0	15	+ 5	+ 7	Ditto	Wind ditto, dark weather, with drifting ſnow.
	4	15	+ 5	+ 6	N. W.	Ditto.
	21	22	+13	+13	Ditto	Ditto.
☿ 23	0	20	+12	+14	Ditto	Ditto.
	4	19	+12	+11½	Ditto	Blowing, ſnowing, drifting, &c.
	21	17	+11½	+11	North	Leſs wind; foggy, with ſleet.
♃ 24	1	30 11	+10	+ 7	Ditto	Thick and dark, wind briſk.
	6	04	+ 6	+ 1	Ditto	Ditto.
	20	29 83	− 3	− 7	N. W.	Cloudy, with little wind.
♀ 25	0	29 78	− 1	− 2	Ditto	Hazy, but clearing faſt.
	4	29 75	− 1	− 2	Ditto	Cloudy, little wind.
	9	29 70	− 2	− 2½	Ditto	Ditto.
	21	29 69	− 4	−16	Ditto	Cloudy, with ſome fog.
♄ 26	0	29 71	− 7½	−12	North	Briſk gale, with a ſtrong haze, and foggy.
	6	29 74	−13	−23	Ditto	Ditto.
						W. W.
	21	37	−20	−20	N. W.	A ſtrong wind and cloudy.
☉ 27	1	29	−17	−16	Ditto	Ditto with drifting.

☉ Nov.

1768.	Ho.	Baro.	Thermome. A	B	Winds.	Weather, &c.
			°	°		
☉ Nov. 27,	9	29 18	— 5	—15	N. W.	Strong wind. A fire in the obſervatory.
	13	14	— 5	—16	Ditto	Ditto, ditto.
	21	14	— 5	—16	Ditto	Ditto, ditto.
☽ 28	0	29 18	— 1	—12	N. W.	Moderate wind and cloudy.
	8	30	— 6	—16	North	Moderate wind, and very hazy.
	16	44	—10	—18½	Ditto	Ditto.
	22	43	—11	—12	Var. fr. N.E. to N. W.	Moderate wind and ſnowing.
♂ 29	0	42	—10	—11	N. W.	Ditto.
	8	43	— 4	— 6	Ditto	Little wind and very hazy.
	16	80	— 6	—10	Ditto	Ditto and clear.
	20	90	— 9	—13	Ditto	Ditto and hazy.
☿ 30	0	89	— 4	—10	Ditto	Ditto, cloudy.
	8	87	— 5	— 9	Ditto	Ditto.
	21	73	+ 4	+ 4	Weſt	Ditto.
♃ Dec. 1	0	75	+ 6	+ 3	North	Ditto, clearing.
	8	69	+ 1	— 7	N. W.	Clear.
	12	65	— 1	— 8	Calm	Hazy.
	20	60	— 3	— 6	S. W.	Cloudy.
♀ 2	0	60	— 1	— 8	South.	Hazy.
	8	54	— 6	—16	Ditto	Clear.
	12	50	— 6	—12	N. E.	Cloudy.
	16	51	— 5	— 8	N. W.	Ditto.
	22	55	— 2	— 8	Ditto	Clear.
♄ 3	1	55	— 1	— 6	Weſt	Ditto.
	8	56	— 2	—10	Ditto	Ditto.
						J. D.
	20	29 37	—10	—17	N. W.	Ditto.
☉ 4	0	36	—10½	—13	Ditto	Ditto.
	4	39	— 8	—12	Ditto	Wind riſing and coming on thick.
	8	40	— 9	—14	Ditto	Thick with ſleet, and the wind high.
	12	45	—10	—17	Ditto	Ditto.

1768.	Ho.	Baro.	Thermome.		Wind.	Weather, &c.
			A °	B °		
☉ Dec. 4, at	16	29 49	— 7½	—17	N. W.	Ditto.
	20	54	—10	—17	Ditto	Hazy, drifting weather.
☽ 5	0	29 61	— 7½	—16½	N. W.	Foggy, and a briſk breeze.
	4	29 64	— 8	—19	North	Ditto.
	8	67	— 8	—22	Ditto	Very hazy, the wind high.
	20	78	—17	—23	Ditto	Ditto.
♂ 6	0	88	—10	—16	N. W.	Ditto.
	4	91	—12	—22	Weſt	Clear and leſs wind.
	20	99	—15	—26	Ditto	Ditto.
☿ 7	0	30 03	—12	—19½	Ditto	Ditto.
	4	05	—10	—21	Ditto	Ditto.
	10	11	—10	—20	Ditto	Hazy and little wind.
	16	16	—11	—18½		Thick and foggy.
	21	20	— 9	—12½	S. E.	Ditto.
♃ 8	0	21	— 6	— 8	Ditto	Ditto.
	4	21	— 1	—14	Ditto	Cloudy.
	20	11	— 2	—13	South	Ditto.
♀ 9	0	08	0	— 9	S. E.	Cloudy and little wind.
	4	03	— 2	—13	Ditto	Ditto.
	21	29 72	—10	—20	South	Ditto and ſome ſleet.
♄ 10	0	68	— 8	—14	Ditto	Ditto.
	5	59	— 8	—21	Ditto	Clear.
	16	51	—20	—32	Weſt	Hazy, a briſk wind.
						——— W. W.
	21	29 51	—24	—37½	Weſt	Ditto.
☉ 11	1	51	—24	—34½	Ditto	Clear.
	8	50	—27	—37½	Ditto	Hazy.
	16	39	—31	—42	Ditto	Ditto.
	21	29 35	—31	—43	Weſt	Thick hazy air.
☽ 12	0	29 31	—34	—39	N. W.	Drifting a little.
	8	29 32	—39	—39	Ditto	Much drift.
	21	29 49	—23	—22	Ditto	Cloudy, with drift.
♂ 13	0	29 53	—21	—20	Ditto	Ditto.
	8	58	—24	—26	Ditto	Ditto.
	21	67	—31	—31	Weſt	Clear and almoſt calm.
☿ 14	3	67	—28	—28	S. W.	Ditto.

☿ Dec.

1768.	Ho.	Baro.	Thermome.		Winds.	Weather.
			A	B		
☿ Dec. 14, at	8	29 68	—30	—33	Weſt	Clear and almoſt calm.
	22	81	—33	—32	Ditto	Clear.
♃ 15	0	87	—29	—22	Ditto	Ditto, little wind.
	10	96	—24	—27	Ditto	Very hazy.
	22	97	—20	—15	N. W.	Cloudy.
♀ 16	0	96	— 8	—17	Ditto	Ditto.
	9	90	—11	—10	North.	Very hazy.
	22	81	—12	— 8	N. W.	Ditto.
♄ 17	2	75	— 9	— 7	N. W.	Cloudy.
	11	70	—10	—11	N. W.	Ditto.
						——————J. D.
	21	71	—10	—10	Ditto	Cloudy, winds briſk.
☉ 18	0	73	—10	— 8	Ditto	Ditto.
	9	78	—11	—10½	Ditto	Ditto.
	20	86	—13	—16	Weſt.	Ditto.
☽ 19	0	88	—11	—15	Ditto	Ditto.
	6	89	—15	—21	Ditto	Very clear and little wind.
	21	85	—13	— 9		Calm, thin clouds.
♂ 20	3	88	—10	—14	N. W.	Hazy, and almoſt calm.
	9	90	—13	—14	Ditto	Ditto.
	20	29 86	—13	—16	South	Thin clouds and little wind.
☿ 21	0	29 78	15	14	Ditto	Very hazy.
	5	72	15	17	S. W.	Ditto.
	20	83	11	13	North	Cloudy, and blows freſh.
♃ 22	8	78	16	18	N. W.	Cloudy, and almoſt calm.
	19	53	14	9	Eaſt	Thick, with drifting ſnow.
♀ 23	0	53	9	3½	N. E.	Ditto.
	8	78	10	11	North	Flying Clouds and light breezes.
	21	86	13	20	Ditto	Cloudy weather.
♄ 24	0	87	16	17	Ditto	Ditto.
	9	92	21	26	N. E.	Ditto.
						——————W. W.

♄ Dec.

1768.	Ho.	Baro.	Thermome.		Winds.	Weather, &c.
			A	B		
♄ Dec. 24, at	22	29 95	—26	—24	Eaſt	Cloudy.
☉ 25	2	80	24	21	E. and variable	Blowing and drifting.
	9	48	18	12	N. E.	Blowing ſtrong and drifting.
	21	57	10	0	N. W.	Ditto.
☽ 26	0				Ditto	Cloudy.
	9	57	8	10	Ditto	Drifting.
	23	30 04	12	8	Ditto	Cloudy, with little wind.
♂ 27	3	05	11	11	Ditto	Ditto.
	12	29 94	14	15	South	Cloudy, with a briſk wind.
	22	53	—11	— 6	Ditto	Blowing ſtrong and drifting.
☿ 28	2	35	— 7	— 1	Ditto	Ditto.
	11	09	+ 2	+11	Ditto	Ditto.
	23	02	+11	+14	S. W.	Hazy, with moderate wind.
♃ 29	3	08	+ 8	+ 8	Ditto	Hazy.
	13	29 10	+11	+17	Calm	Cloudy.
	23	08	+10	+12	North	Snowing, with little wind.
♀ 30	4	12	+ 9	+ 5	Ditto	Snowing, and moderate wind.
	13	29	— 4	—12	Ditto	Cloudy and ditto.
	22	39	—12	—18	N. W.	Hazy and ditto.
♄ 31	5	49	—15	—25	Ditto	Clear and riſing wind.
	12	62	—25	—34	Ditto	Drifting and a briſk wind.
						—J. D.
	22	80	—28	—27	Ditto	Clear.
1769. ☉ Jan. 1.	1	84	—26	—25	Ditto	Ditto.
	6	94	—25	—29	Ditto	Hazy, and briſk wind.
	19	30 11	—25	—27	Ditto	Ditto.
☽ 2	0	18	—26	—26	Ditto	Clear, and leſs wind.
	7	19	—28	—34	North	Ditto.
	21	09	—34	—34	Ditto	Ditto.

♂ Jan.

1769.	Ho.	Baro.	Thermome.		Winds.	Weather.
			A	B		
♂ Jan. 3. at	0	30 03	—29	—24	N. W.	Hazy, almoſt calm.
	6				Weſt	Thick and a briſk wind.
	21	29 57	—18	—16	S. W.	Ditto.
☿ 4	1	40	—16	— 9	Ditto	Ditto.
	5	33	—13	— 9	Weſt	Small drifting ſnow, and the wind high.
	21	53	—19	—24	N. W.	Wind very high, and thick drifting weather.
♃ 5	1	57	—21	—23	Ditto	Ditto.
	6	63	—24	—26	North	Ditto.
	21	82	—28	—29	N. W.	Ditto.
♀ 6	1	91	—27	—29	Ditto	Ditto. The ſpirits of wine, in which the plumb of the quadrant is immerſed, is now congealed to the conſiſtence of a common jelly. I apprehend this was done in the night; but I did not obſerve it before.
	6	29 94	—28	31	N. W.	Hazy, and the wind very high.
	21	98	—32	35	N. W.	Cloudy, the wind briſk.
♄ 7	1	30 00	—29	29	Ditto	Foggy, and leſs wind.
	6	30 00	—33	37	Ditto	Hazy, wind ditto.
						W. W.
	22	29 87	—33	—31	Weſt	Hazy. wind moderate.
☉ 8	1	84	—30	—25	Ditto	Clear, ditto.
	8	70	—30	—33	S. W.	Ditto, little wind.
	22	40	—28	—21	Ditto	Very hazy, ditto.
☽ 9	2	35	—24	—19	Ditto	Ditto.
	9	39	—23	—21	Weſt	Hazy, ditto.
	21	40	—31	—30	Ditto	Clear, ditto.
♂ 10	2	48	—27	—25	Ditto	Hazy wind riſing.
	11	59	—27	—29	Ditto	Ditto, wind moderate.

♂ Jan.

	1760.	Ho.	Baro.	Thermome. A	Thermome. B	Winds.	Weather, &c.
♂	Jan. 10, at	22	29 76	—28	—15	North	Hazy. wind moderate.
☿	11	2	82	—23	—18	Weſt	Very hazy, ditto.
		11	97	—26	—30	N. W.	Clear, ditto.
		22	30 00	—32	—30	Weſt	Ditto.
♃	12	0	29 96	—30	—28	Ditto	Ditto.
		11	78	—26	—25	Ditto	Very hazy, ditto.
		22	54	—24	—17	S. W.	Clear, ditto.
♀	13	2	42	—19	—13	South.	Ditto.
		11	3[illegible]	—20	—20	S. W.	Ditto.
		22	4[illegible]	—19	—21	Weſt	Clear.
♄	14	1	44	—20	—21	Weſt	Ditto.
		11	46	—28	—30	Ditto	Hazy, wind riſing.
							——J. D.
		21	29 4[illegible]	—27	—27	N. W.	Thick, with ſmall drifting ſnow; wind high.
☉	15	1	45	—26	—21	Ditto	Ditto.
		6	51	—26	—28	Ditto	Hazy and leſs wind.
		20	52	—31	—31	Ditto	Ditto.
☽	16	0	52	—28	—22	Weſt	Tolerably clear, briſk wind.
		5	52	—25	—28	N. W.	A little hazy.
		18	48	—32	—36	Ditto	Ditto.
		21	47	33	—35	Ditto	Clear.
♂	17	1	47	—31	—31	Ditto	Very hazy.
		8	4[illegible]	—34	—39	Ditto	Ditto.
		20		—39	—40½	Ditto	Clear.
		22	29 46	—39	—38	Ditto	Ditto.
☿	18	1	47	—36	—32	Ditto	Ditto.
		9	36	—34	—36	S. W.	Very hazy, and briſk wind.
		21	20	—23	—30	Weſt	Thick and little wind.
♃	19	1	18	—26	—12	Ditto	Very hazy.
		5	16	—21	—17	North.	Thick, with ſleet and little wind.
		20	34	—28	—31	N. W.	Very hazy.
♀	20	1	37	—26	—24	Ditto	Clear, the wind briſk.
		6	45	—27	—30	Ditto	Ditto.
		8	5[illegible]	—33	—36	Ditto	Ditto.

1769.	Ho.	Baro.	Thermome. A	B	Winds	Weather, &c.
♄ Jan. 21, at	0	29 64	−33	−29	N. W.	Clear, the wind briſk.
	5	74	−31	−36	Ditto	Ditto.
						W. W.
	21	29 80	−36	−36	Weſt	Clear, wind moderate.
☉ 22	2	85	−31	−21	Ditto	Very hazy, ditto.
	9	91	−30	−31	N. W.	Very thick haze, ditto.
	16	91	−36	−45	Ditto	Clear, the cold very ſharp; whilſt I only looked at the thermometer B, it began to freeze my noſe; a briſk wind.
	21	93	−43	−41	Weſt	Clear, ditto.
☽ 23	1	95	−37	−33	N. W.	Ditto.
	10	96	−37	−38	Weſt	Ditto, the wind leſs.
	21	87	−37	−36	Ditto	Ditto.
♂ 24	1	85	−33	−28	Ditto	Ditto.
	10	79	−31	−31	Ditto	Cloudy, with ſome drift.
	22	77	−23	−19	N. E.	Snowing.
☿ 25	1	75	−21	−14	North	Drifting, the wind ſtronger.
	10	73	−19	−17	Ditto	Ditto, leſs wind.
	22	77	−19	−16	Ditto	Snowing briſkly.
♃ 26	1	82	−19	−21	N. W.	Ditto.
	10	87	−23	−24	Ditto	Cloudy, and ſtrong wind.
	22	93	−25	−25	Ditto	Ditto.
♀ 27	1	97	−25	−20	Ditto	Ditto.
	10	30 00	−29	−31	Ditto	Clear, blowing and drifting.
	22	17	−34	−34	Ditto	Ditto, a briſk wind.
♄ 28	1	20	−31	−28	Ditto	Ditto.
	12	22	−34	−38	Ditto	Hazy, and leſs wind.
						J. D.
	20	30 13	−37	−40	North	Clear, and briſk wind.
☉ 29	0	10	−35	−30	N. W.	Ditto.
	6	07	−30	−35	Ditto	Ditto.
	20	29 98	−36	−41	Ditto	Ditto.

☽ 30

1769.	Ho.	Baro.	Thermome.		Winds.	Weather, &c.
			A	B		
☽ Jan. 30, at	0	29 97	—34	—30	N. W.	Clear and briſk wind.
	6	96	—28	—32	S. W.	Hazy, and almoſt calm.
	20	99	—26	—20	N. W.	Thick, with ſleet; wind briſk.
♂ 31	0	30 07	—24	—18	Ditto	A little foggy, the wind ditto.
	6	17	—24	—31	Ditto	Clear, the wind briſk.
	20	09	—31	—27	S. W.	Cloudy, the wind moderate.
☿ Feb. 1	0	30 00	—27	—14	South	Ditto.
	6	29 75	—22	—23	S. E.	Very thick, the wind high.
	20	32	—11	— 6	Weſt	Thick, the wind moderate. N. B. A large quantity of ſnow has fallen in the night.
♃ 2	0	29 40	— 9	— 1	N. W.	A little hazy, the wind briſk.
	6	63	—14	—21	Ditto	Wind briſk, and much drift, cauſed by the new fallen ſnow lying looſe.
	20	30 03	—27	—30	N. W.	Very hazy, the wind high and drifting.
♀ 3	0	07	—26	—24	Ditto	Clear, and leſs wind.
	6	29 90	—24	—26	S. E.	Cloudy, the wind moderate.
	20	02	—14	— 7	Ditto	Very thick, with ſleet, the wind ditto.
♄ 4	0	28 89	—12	— 0	N. W.	Ditto, the wind briſk.
	6	29 37	—14	—19	Ditto	Ditto.
						————W. W.
	22	29 68	—23	—20	Weſt	Clear, with a briſk wind.
☉ 5	0	71	—20	—15	Ditto	Ditto, ditto.
	8	79	—16	—20	N. W.	Ditto.
	16	88	—21	—26	Dttio	Hazy, and the wind ſtrong.

☽ Feb.

1769.	Ho.	Baro.	Thermome. A	B	Winds.	Weather, &c.
☽ Feb. 6, at	0	29 95	—21	—16	N. W.	A little hazy, and leſs wind.
	8	94	—21	—23	Ditto	Very thick, the wind moderate.
	15	91	—21	—24	Calm	Cloudy.
♂ 7	0	91	—21	— 3	S. W.	Ditto, with little wind.
	10	97	—16	—19	Calm	Hazy, with ſleet.
	15	30 04	—19	—19	Ditto	Ditto.
☿ 8	0	30 09	—15	—10	S. W.	Clear, with little wind.
	8	30 03	—13	—13	South	Ditto.
	16	29 87	—13	—10	Ditto	Hazy, and wind briſk.
♃ 9	0	77	— 5	+15	S. W.	Clear, wind moderate.
	8	59	+ 9	+12	Ditto	Coming on hazy, and little wind.
	15	61	+ 7	+12	N. W.	Cloudy, ſtrong wind, and drifting.
♀ 10	0	93	— 1	— 0	North	Dittto.
	9	30 22	—10	—16	Ditto	Clear, ditto.
	16	30 32	—16	—21	Ditto	Ditto, and leſs wind.
	20	30 33	—21	—26	Ditto	Ditto, ſtill leſs wind.
♄ 11	1	30 30	—10	—15	Weſt	Ditto, the wind moderate.
	13	29 82	—13	—13	S. E.	Cloudy, little wind, with ſleet.
						J. D.
	10	29 86	—10	— 9	North	Thin clouds, the wind moderate.
☉ 12	0	96	— 7	— 3	N. W.	Clear, the wind ditto.
	6	99	—11	—21	Weſt	Cloudy and little wind.
	20	99	—22	—26	N. W.	The wind very high, and much drift.
☽ 13	1	30 04	—19	— 9	Ditto	Ditto.
	6	07	—20	—25	Ditto	Ditto.
	20	29 93	—26	—27	Ditto	Clear, the wind briſk.
♂ 14	0	94	—20	—15	Ditto	Ditto.
	6	98	—17	—21	Ditto	Ditto.
	20	61	—23	—18	South	Thick, and the wind moderate.

1769.		Ho.	Baro.	Thermome. A	B	Winds.	Weather, &c.
☿ Feb.	15	1	29 31	—17	+ 3	S. E.	Snowing briſkly, and little wind.
		6	21	—10	— 2	Ditto	Thick, with ſleet, and little wind.
		20	56	—11	—11	North	Cloudy, the wind moderate.
♃	16	0	66	— 7	— 6	Ditto	Clear, wind as above.
		6	69	— 9	—16	Ditto	Thin clouds, and little wind.
		20	57	—16	—16	Ditto	Thin flying clouds, and little wind.
♀	17	0	58	—10	— 2	S. W.	Ditto and foggy.
		6	58	—10	—19	Weſt	Cloudy, little wind.
		20	76	—17	—20	North	Ditto.
♄	18	0	80	—16	— 4	Ditto	Ditto.
		6	72	—15	—19	Ditto	Thick, with ſleet, the ſhind briſker.
							———W. W.
		21	29 68	—20	—18	N. E.	Clear, with a briſk wind.
☉	19	0	70	—16	—10	Ditto	Ditto.
		8	70	—17	—18	Ditto	Cloudy, ditto.
		20	61	—15	— 8	Ditto	Snowing, with moderate wind.
☽	20	0	53	—11	+ 5	Eaſt	Ditto.
		8	49	— 9	—12	North	Clear, ditto.
		14	59	—10	— 8	Ditto	Cloudy, with little wind.
♂	21	1	72	— 9	+ 6	N. W.	Ditto.
		9	81	—11	—20	Ditto	Clear, with moderate wind.
		20	82	—26	—32	Weſt	Ditto.
☿	22	0	79	—21	—21	Ditto	Ditto.
		10	68	—23	—30	Ditto	Hazy, ditto.
		20	67	—31	—41	North	Ditto, blowing ſtrong and drifting.
♃	23	0	71	—33	—32	N.W.	Cloudy, ditto.
		8	72	—35	—38	Ditto	Clear, leſs wind, but drifting.

1769.	Ho.	Baro.	Thermo. A	B	Winds.	Weather, &c.
♃ Feb. 23, a	20	29 63	—33	—29	Weſt	Cloudy, with moderate wind.
♀ 24	1	68	—26	—10	Ditto	Clear, ditto.
	8	85	—20	—29	N. W.	Ditto.
	15	93	—29	—37	S. W.	Coming on thick, with a briſk wind.
	21	95	—35	—36	Weſt.	Clear, and moderate wind.
♄ 25	0	95	—30	—24	Ditto	Ditto.
	8	90	—25	—31	Ditto	Ditto.
	15	80	—31	—35	N. W.	Cloudy, with a briſk wind.
						——— J. D.
	20	29 71	—34	—38	N. W.	Clear, and the wind briſk.
☉ 26	1	71	—29	—24	Ditto	Hazy, ditto.
	6	73	—30	—35	Ditto	Thin clouds, wind high.
	20	75	—38	—42	Ditto	Clear, and moderate wind.
☽ 27	0	80	—34	—28	Ditto	Ditto.
	6	86	—28	—33	Ditto	Thin flying clouds, ditto.
	20	80	—38	—37	Weſt	Hazy, and little wind.
♂ 28	0	80	—30	— 9	Ditto	Ditto.
	6	81	—17	—12	N. W.	Ditto.
	20	90	—27	—30	Ditto	Ditto, the wind moderate.
☿ March 1.	0	92	—27	—18	Ditto	Ditto, with ſleet, or drift.
	6	98	—25	—34	Ditto	Wind high, and drifting.
	20	97	—39	—41	Ditto	Clear, and the wind high.
♃ 2	0	98	—31	—25	Ditto	Ditto.
	6	30 02	—28	—31	Ditto	Clear, the wind briſk.
	15	29 96	—30	—33	Ditto	A little hazy, the wind moderate.

♃ March

1769.	Ho.	Baro.	Thermome.		Winds.	Weather, &c.
			A	B		
♃ March 2, at	21	29 94	—31	—25	Weſt	Clear, and the wind moderate.
♀ 3	0	92	—25	—13	Ditto	Thin haze, the wind ditto.
	6	86	—18	—15	Ditto	Clear, and little wind.
	20	68	—29	—30	Ditto	Hazy, wind moderate.
♄ 4	0	65	—21	—10	Ditto	Clear, ditto.
	6	63	—14	—18	Ditto	Hazy, and almoſt calm.
						——W. W.
	21	29 68	—31	—32	N. W.	Clear, with moderate wind.
☉ 5	0	29 77	—29	—26	Weſt	Clear, the wind briſk, and low drift.
	8	83	—31	—33	Ditto	Very hazy, the wind briſk, and no drift.
	21	80	—36	—33	Ditto	Clear, and leſs wind.
☽ 6	0	82	—27	—19	Ditto	Clear, and the wind briſk.
	7	80	—24	—30	Ditto	Clear, ditto.
	18	69	—36	—41	Ditto	Ditto, wind moderate.
♂ 7	0	66	—30	—20	Ditto	Hazy, wind briſk.
	9	75	—30	—35	N. W.	Clear, ditto.
	21	80	—35	—27	Ditto	Cloudy, ditto, and drifting.
☿ 8	0	90	—30	— 5	Ditto	Ditto, with leſs wind.
	10	30 21	—27	—28	Ditto	Clear, blowing ſtrong, and drifting.
	21	30 24	—36	—28	Weſt	Very hazy, the wind briſk.
♃ 9	0	30 14	—30	—11	S. W.	Ditto.
	10	29 88	—17	—11	Ditto	Clear, ditto, with low drift.
	20	50	— 8	+ 6	Ditto	Cloudy, and the wind ſtrong.
♀ 10	2	44	— 0	+22	Ditto	Ditto.
	10	63	— 0	— 3	N. W.	Ditto, and drifting.
	21	81	—11	—14	Ditto	Ditto.
♄ 11	0	85	—10	— 8	Weſt	Clear, ditto.

♄ March

1769.	Ho.	Baro.	Thermome. A	B	Winds.	Weather.
♄ Mar. 11, a	11	29 86	—18	—23	N. W.	Clear, and leſs wind.
						————J. D.
	20	29 77	—27	—22	S. W.	Clear, and briſk wind.
☉ 12	0	71	—16	— 0	Ditto	Ditto, little wind.
	6	54	— 7	— 5	Ditto	Cloudy, almoſt calm.
	20	26	—17	—23	N. W.	Thick, the wind high, and much drift.
☽ 13	1	72	—21	— 9	N. W.	Cloudy, wind briſk.
	6	77	—17	—19	Ditto	Clear, the wind briſk, with drift.
	20	69	—26	—26	Ditto	Ditto, the wind briſk.
♂ 14	1	72	—19	— 6	Ditto	Hazy, and almoſt calm.
	6	71	—15	—16	Ditto	Ditto.
	20	63	—28	—30	Ditto	Clear, wind moderate.
☿ 15	0	70	—21	—16	Ditto	Ditto.
	6	77	—18	—21	Ditto	Ditto.
	20	59	—28	—25	Ditto	Ditto, wind high.
♃ 16	0	60	—23	—10	Ditto	Ditto, with drift.
	6	67	—18	—22	Ditto	Clear, wind moderate.
	20	59	—22	—16	Ditto	Ditto.
♀ 17	0	66	—14	+ 2	Ditto	Thin clouds, wind moderate.
	6	76	— 6	— 1	Ditto	Clear, wind briſk.
	20	90	—19	—21	Ditto	Clear, wind moderate.
♄ 18	0	92	—13	+ 1	Ditto	Hazy, wind ditto.
	6	89	— 5	+ 1	Weſt.	Cloudy, and little wind.
						————W. W.
	20	29 70	— 4	+ 7	Weſt	Very hazy, and little wind.
☉ 19	0	59	+ 6	+20	Ditto	Clear, and ditto.
	8	45	+ 1	— 0	N. E.	Very hazy, with briſk wind.
	20	48	— 0	+ 3	North	Cloudy, and a ſtrong wind.
☽ 20	0	65	— 0	+11	Ditto	Cloudy, and the wind ſtrong.
	8	80	— 1	— 8	Ditto	Clear, ditto, with a little drift.

☽. Mar.

1769.	Ho.	Baro.	Thermome.		Wind.	Weather, &c.
			A	B		
☽ Mar. 20, a.	20	29 78	—16	— 8	Weſt	Clear, ditto, with little drift.
♂ 21	1	83	— 7	+ 4	Ditto	Ditto.
	8	80	— 4	— 8	Ditto	Ditto, leſs wind.
	20	63	—15	—11	Ditto	Ditto, wind moderate.
☿ 22	0	50	— 5	+ 5	N. W.	Ditto.
	8	57	— 5	—12	Ditto	Ditto.
	20	66	—22	—14	Ditto	Ditto, the wind ſtrong.
♃ 23	0	80	—13	— 8	Ditto	Ditto.
	9	93	—18	—24	Ditto	Ditto, wind moderate.
	20	30 01	—25	—16	Ditto	Ditto.
♀ 24	0	30 07	—13	— 5	Ditto	Ditto.
	9	30 10	—13	—21		Hazy and calm.
	15	30 13	—19	—23	Ditto	Ditto, and little wind.
♄ 25	0	30 19	— 8	+ 6	Weſt	Clear ditto.
	9	30 21	— 8	—15	Ditto	Ditto.
						————J. D.
	21	30 18	—15	— 3	South	Clear, and little wind.
☉ 26	0	30 13	— 4	+14	S. E.	Ditto.
	6	29 96	+ 2	— 0	Ditto	Cloudy, wind moderate.
	19	58	+ 4	+15	South	Ditto, wind briſk.
☽ 27	0	49	+11	+40	Ditto	Ditto, wind moderate.
	7	47	+24	+24	Ditto	Clear, and very little wind.
	19	47	+ 9	+11	South	Hazy, and almoſt calm.
♂ 28	0	44	+24	+37	Eaſt	Ditto.
	6	45	+35	+23	N. W.	Cloudy, the wind as above.
	19	62	+12	+ 5	North	Cloudy, the wind briſk with drift.
☿ 29	0	74	+10	+10	Ditto	Hazy, wind briſk.
	6	82	+ 9	+ 9	N. W.	Clear, wind moderate.
	19	79	—11	— 6	Ditto	Very hazy, and almoſt calm.
♃ 30	0	71	— 2	+20	Eaſt	Thick with ſnow, and little wind.
	6	63	+ 4	+ 7	N. E.	Ditto, wind briſk.
	20	79	+ 1	+ 8	Ditto	Hazy, ditto.

♀ Mar.

1769.	Ho.	Baro.	Thermome. A	Thermome. B	Winds.	Weather.
♀ Mar. 31, at	0	29 82	+ 8	+14	North.	Clear, wind briſk.
	7	81	+11	+ 5	Eaſt.	Ditto, little wind.
	20	31	+12	— 6	South.	Blowing ſtrong, and much drift.
♄ April 1.	0	18	+01	+31	Ditto	Snowing, wind high, and much drift.
	6	11	+13	+26	Ditto	Cloudy, almoſt calm.
						W. W.
	19	29 24	+ 9	+12	North.	Snowing and blowing briſk.
☉ 2	0	37	+10	+23	Weſt	Ditto, and blowing very ſtrong.
	6	56	+ 7	+ 5	Ditto	Drifting, and ditto.
	13					Ditto.
	19	84	— 9	— 8	Ditto	Clear, leſs wind, and but little drift.
☽ 3	0	93	— 2	+ 7	N. W.	Clear, briſk wind, and low drift.
	9	30 02	+ 1	— 7	Ditto	Ditto, and ditto.
	13	30 05	— 5	—12	Ditto	Ditto.
	20	30 10	— 9	— 4	Ditto	Ditto, wind moderate.
♂ 4	0	30 12	— 2	+ 8	North	Ditto.
	9	30 15	+ 4	+ 3	Ditto	Cloudy, ditto.
	19	30 12	— 7	— 7	N. W.	Clear, ditto.
☿ 5	0	30 13	+ 3	+16	Ditto	Ditto.
	8	30 12	+ 9	— 1	Weſt	Ditto, little wind.
	19	30 08	— 2	+ 5	North	Ditto, wind moderate.
♃ 6	0	30 10	+ 9	+20	Ditto	Ditto, the wind briſk.
	9	30 14	+ 5	— 2	Ditto	Ditto, wind moderate.
	19	30 08	0	0	Ditto	Thin clouds, and the wind briſk.
♀ 7	0	30 03	+ 5	+23	Ditto	Cloudy, and the wind ſtrong.
	9	30 11	+ 5	+ 6	N. E.	Ditto, wind moderate.
	20	30 24	+ 1	+20	Ditto	Thin clouds, and little wind.
♄ 8	0	30 26	+15	+26	Ditto	Ditto, wind briſk.
	7	30 19	+20	+14	Ditto	Cloudy, little wind.
						J. D.

1769.	Ho.	Baro.	Thermome.		Wind.	Weather, &c.
			A	B		
♄ April 8, at	19	30 0	+ 6	+12	Weſt	Hazy, almoſt calm.
☉ 9	0	29 99	17	33	N. W.	Ditto.
	7	99	19	12	Ditto	Ditto.
	19	92	3	11	Ditto	Very hazy, with ſleet.
☽ 10	0	94	13	26	Ditto	Thin flying clouds, the wind moderate.
	8	30 10	11	6	Ditto	Thick, with ſleet, the wind briſk.
	19	30 20	— 8	— 6	Ditto	Clear, ditto.
♂ 11	0	30 24	+ 3	+11	Ditto	Ditto.
	6	30 24	+ 8	+17	Ditto	Ditto, wind moderate.
	19	30 07	— 8	— 0	Ditto	Ditto.
☿ 12	0	29 98	+11	+19	Eaſt	Ditto.
	6	29 84	13	15	Ditto	Thick, with ſnow, and little wind.
	19	29 73	5	12	N. W.	Clear, wind moderate.
♃ 13	0	29 71	14	23	Ditto	Ditto, little wind.
	6	29 60	21	13	S. E.	Thin flying clouds, and little wind.
	8				Ditto	Cloudy.
	20	29 37	9	23	South	Ditto, with ſnow, the wind moderate.
♀ 14	0	29 39	14	45	Eaſt	Ditto.
	6	29 51	21	20	Eaſt	Ditto.
	19	29 70	3	7	Eaſt	Very thick, the wind moderate.
♄ 15	0	29 83	9	29	Ditto	Thick, wind as above.
	6	29 95	12	9	N. W.	Cloudy, ditto.
						—W. W.
	20	29 91	+ 1	+ 5	N. W.	Clear, wind briſk.
● 16	0	88	9	14	Ditto	Ditto, wind moderate.
	9	85	7	0	N. E.	Cloudy, wind briſk.
	19	79	2	8	North	Ditto.
☽ 17	0	80	15	26	Ditto	Cloudy, wind ſtrong and drifting.
	5	79	15	21	Ditto	Hazy, wind ditto, and drifting.
	9	75	11	10	Ditto	Very thick, ditto.
	19	58	6	15	Ditto	Ditto.

1769.	Ho.	Baro.	Thermome.		Winds.	Weather, &c.
			A	B		
♂ April 18, at	1	29 59	+15	+34	North	Very thick, leſs wind and drift.
	5	64	17	26	Ditto	Ditto, wind the ſame.
	9	68	17	20	Ditto	Ditto, more wind.
	20	81	18	27	Ditto	Ditto, with leſs wind.
☿ 19	0	90	23	44	Ditto	Ditto, the wind briſk.
	8	99	24	31	Ditto	Ditto, wind moderate.
	12	30 03	22	20	Ditto	Ditto, little wind.
	19	30 03	19	27	Eaſt	Ditto.
♃ 20	1	04	31	47	N. E.	Ditto.
	9	29 98	30	22	Calm	Cloudy.
	19	84	19	31	N. W.	Snowing, and very little wind.
♀ 21	0	84	31	38	Ditto	Clear, and little wind.
	9	76	30	17	Eaſt	Ditto, and very little wind.
	20	64	13	20	Calm	Thick fog.
♄ 22	0	63	23	28	N. W.	Clear, and very little wind.
	9	57	29	21	Ditto	Cloudy, ditto.
						—J. D.
	19	29 46	+21	+27	Weſt	Very thick, and little wind.
☉ 23	0	44	31	39	Ditto	Flying clouds, the wind as above.
	6	46	37	37	N. W.	Clear, and little wind.
	19	60	26	29	Eaſt	Flying clouds, the wind moderate.
☽ 24	2	69	37	36	Ditto	Cloudy, little wind.
	7	69	32	26	Ditto	Hazy, ditto.
	19	71	19	26	S. E.	Cloudy, the wind moderate.
♂ 25	0	73	27	47	South	Ditto.
	6	74	36	37	S. E.	Clear, the wind as above.
	19	68	26	31	Ditto	Flying clouds, the wind briſk.
☿ 26	5	59	43	45	Ditto	Very hazy, the wind moderate.

1769.	Ho.	Baro.	Thermome. A	Thermome. B	Winds.	Weather, &c.
☿ April 26, at	19	29 59	+35	+40	S. W.	Thick, and the wind briſk.
♃ 27	0	75	43	48	Ditto	Clear, wind moderate.
	6	88	51	56	Ditto	Clear, and little wind.
	19	92	33	34	Eaſt	Thin clouds, and the wind moderate.
♀ 28	0	93	42	47	Ditto	Hazy, ditto.
	6	30 00	45	40	North	Cloudy, ditto.
	19	09	27	28	N. E.	Ditto, the wind briſk.
♄ 29	0	10	32	32	Ditto	Very hazy.
	6	07	32	28	Ditto	Thick, the wind briſk.

——— W. W.

	21	29 94	+24	+28	Ditto	Cloudy, wind briſk.
☉ 30	0	92	26	36	Ditto	Ditto, ditto.
	9	89	28	29	Ditto	Ditto, ditto.
	20	86	27	32	Ditto	Snowing, the wind moderate.
☽ May 1	3	90	35	45	Ditto	Cloudy, and little wind.
	9	94	34	29	North	Ditto, ditto.
	20	30 04	29	35	N. E.	Ditto, the wind moderate.
♂ 2	2	10	36	46	Ditto	Thick fog, ditto.
	9	07	35	28	North.	Thin clouds and little wind.
	20	04	29	38	S. E.	Ditto, ditto.
☿ 3	0	04	41	45	Ditto	Clear, ditto.
	9	12	40	32	Eaſt	Ditto.
	20	20	32	37	Ditto	Ditto.
♃ 4	0	22	42	43	Ditto	Ditto.
	9	18	41	36	South.	Ditto.
	20	16	39	46	Ditto	Ditto, the wind moderate.
♀ 5	1	09	50	59	Ditto	Ditto.
	9	29 94	55	54	Ditto	Ditto.
	20	98	50	45	North	Cloudy, wind briſk.
♄ 6	1	30 08	47	50	Ditto	Hazy, wind moderate.
	9	08	44	35	S. E.	Clear, and little wind.

——— J. D.

1769.	Ho	Baro.	Thermome.		Winds.	Weather, &c.
			A	B		
♄ May 6, at	19	29 97	+33	+39	Weſt	Cloudy, and little wind.
☉ 7	1	29 96	41	45	North	Clouds, the wind moderate.
	6	93	39	34	Eaſt	Ditto, raining briſkly.
	19	91	32	35	Ditto	Cloudy, wind briſk.
☽ 8	0	88	35	39	Ditto	Very thick and foggy; the wind briſk.
	6	82	36	38	Ditto	Cloudy, and the wind moderate.
	19	75	32	37		A thick wet fog, and calm.
♂ 9	0	83	37	49	N. W.	Cloudy, little wind.
	6	98	47	45	Ditto	Flying clouds, and the wind briſk.
	19	30 23	28	26	North	Clear, the wind high.
☿ 10	0	29	34	32	Ditto	Ditto, ditto.
	6	27	34	29	Eaſt	Cloudy, little wind.
	19	29 88	26	28	South	Ditto, the wind briſk.
♃ 11	0	75	28	35	Ditto	A briſk wind, with rain.
	6	62	32	39	S. W.	Cloudy, and almoſt calm.
	19	96	24	21	N. W.	Thin clouds, the wind very high.
♀ 12	0	30 06	26	23	North	Ditto, ditto.
	6	30 14	26	24	Ditto	Clear, and leſs wind.
	20	30 14	15	22	N. W.	Thick, with ſnow, the wind high.
♄ 13	0	30 14	22	27	Ditto	Cloudy, wind briſk.
	7	30 08	25	20	Ditto	Hazy, with flying clouds.
						———W. W.
	20	29 94	+11	27	Eaſt	Cloudy, and little wind.
☉ 14	1	90	24	42	South	Ditto, a briſk wind.
	8	81	31	28	Eaſt	Ditto, ditto.
	20	74	26	33	S. E.	A briſk fall of hali, and moderate wind.

1769.	Ho.	Baro.	Thermome.		Winds.	Weather, &c.
			A	B		
☽ May 15, at	1	29 81	+30	+40	Eaſt	Snowing, and blowing briſkly.
	9	30 02	22	27	N. E.	Cloudy, the wind ditto.
	20	30 10	20	32	North	Ditto, the wind moderate.
♂ 16	6	30 12	32	34	Ditto	Ditto, the wind briſk.
	10	30 11	29	24	Ditto	Ditto, ditto.
	20	30 08	25	35	Ditto	Ditto.
☿ 17	0	30 10	29	47	Ditto	Ditto.
	10	30 11	32	31	Ditto	Ditto.
	20	30 10	31	38	N. W.	A thick wet fog, and moderate wind.
♃ 18	1	30 10	36	54	Weſt	Cloudy, and very little wind.
	10	30 00	37	32	South	Coming on very hazy; little wind.
	20	29 86	35	44	Weſt	Cloudy, and moderate wind.
♀ 19	2	85	38	50	Nor'ly	Cloudy, and ditto.
	9	90	35	32	North	Ditto, blowing ſtrong.
	20	89	29	33	Ditto	Ditto, the wind moderate.
♄ 20	1	85	32	39	N. W.	Clear, the wind ditto.
	9	83	36	28	Nor'ly	Cloudy, and little wind.
						J. D.
	20	29 79	+27	+33	North	Thick, with ſleet, little wind.
☉ 21	0	80	31	48	Ditto	Ditto, ditto.
	6	80	38	40	Calm	Ditto.
	19	76	30	36	Ditto	Ditto.
☽ 22	0	76	41	42	South	Clear, a moderate wind.
	6	72	40	41	Eaſt	Cloudy, ditto.
	18	70	30	35	Ditto	Ditto, ditto.
♂ 23	0	62	38	43	North	Ditto.
	6	76	39	38	Ditto	Ditto.

1769.	Ho.	Baro.	Thermome. A	B	Winds.	Weather, &c.
♂ May 23, at	19	29 80	+26	+30	N. W.	Very thick; moderate wind, much ſnow in the night.
☿ 24	0	87	36	43	Ditto	Hazy, and little wind.
	6	87	42	47	Ditto	Ditto, ditto.
	19	84	29	33	N. W.	Clear, ditto.
♃ 25	0	93	42	51	N. W.	Cloudy, a briſk wind.
	6	98	41	36	Ditto	Ditto, ditto.
	19	85	30	42	S. W.	Ditto.
♀ 26	0	78	43	57	Ditto	Ditto, little wind.
	6	64	46	56	Ditto	Hazy, ditto.
	19	73	34	39	N. W.	Thick, wind briſk.
♄ 27	0	78	39	48	North	Flying clouds, wind as above.
	8	80	42	36	Eaſt	Cloudy, and little wind.
						———W. W.
	20	29 76	+31	+34	N. E.	Thick ſettled clouds, and moderate wind.
☉ 28	0	78	35	45	Ditto	The ſun at times ſhining faintly.
	8	81	40	37	North	Thin clouds, and little wind.
	19	78	31	34	Ditto	Clear, and little wind.
☽ 29	0	83	44	49	Eaſt	Broken clouds, and little wind.
	10	85	41	34	North	Thick clouds, the wind ditto.
	19	29 88	34	36	N.W.	Very clear, and little wind.
♂ 30	5	89	49	49	Eaſt	Hazy, wind moderate.
	9	87	42	34	N. E.	Thin clouds, and very little wind.
	19	78	34	37	Eaſt	Snowing very faſt; ditto wind.
☿ 31	1	83	45	49	N.W.	Cloudy, and moderate wind.
	9	89	42	43	Weſt	Clear, ditto.

☿ May

			Thermome.			
1769.	Ho.	Baro.	A	B	Winds.	Weather, &c.
☿ May 31, at	19	29 96	+33	+40	Weſt	Thin clouds, and little wind.
♃ June 1	1	96	46	49	Eaſt	Cloudy, a briſk wind.
	4	89	44	45	North	Raining briſkly.
	9	74	37	34	N. E.	Raining, and blowing ſtrong.
	20	50	35	40	Weſt	Drizzling rain, and little wind.
♀ 2	0			45		Clear.
	5	80	49	51	S. W.	Ditto.
	10	90	39	44	Weſt	Ditto, and very little wind.
	18	96	33	37	South	Clear.
	22	95	43	55	Ditto	Clear at times, flying clouds.
♄ 3	0	96	50	60	Ditto	More flying clouds, the wind riſing.
						J. D.
	19	29 65	+38	+42	South	Hazy, wind moderate.
☉ 4	0	29 60	49	56	Ditto	Some flying clouds, the wind as above.
	8	55	49	42	Ditto	Ditto.
	19	47	36	40	Calm	Clear.
☽ 5	1	46	52	59	S. W.	Cloudy, the wind moderate.
	7	46	49	47	N. W.	Flying clouds, the wind briſk.
	19	37	36	40	Ditto	Ditto, wind very high.
♂ 6	1	36	39	45	Ditto	Thick, the wind as above.
	6	42	38	36	Ditto	Ditto, with rain, ditto.
	19	49	34	38	Ditto	Cloudy, ditto.
☿ 7	0	53	37	45	Ditto	Hazy, the wind briſk.
	6	54	44	46	Ditto	Cloudy, little wind.
	19	37	33	40	N. E.	Ditto.
♃ 8	0	34	43	49	North	Thin clouds, ditto.
	6	34	48	47	Ditto	Cloudy, ditto.
	19	32	36	40	N. W.	Ditto, the wind moderate.

1769.	Ho.	Baro.	Thermome. A	B	Wind.	Weather, &c.
♀ June 9, at	0	29 39	+42	+50	N. W.	Cloudy, and very little wind.
	6	39	44	44	Ditto	Ditto, moderate wind.
	19	37	35	40	Ditto	Very thick, the wind briſk.
♄ 10	0	42	42	50	Ditto	Flying clouds, the wind moderate.
	7	46	50	45	Ditto	Cloudy, ditto.
						——— W. W.
	19	29 49	+36	+40	N. W.	Some flying clouds, and moderate wind.
☉ 11	0	59	49	53	N. W.	Moſtly cloudy, the wind as above.
	9	68	52	59		Clear, and little wind.
	19	71	38	39	Eaſt	Moſtly cloudy, and likely for rain.
☽ 12	4	79	43	41	Ditto	Clearing a little.
	10	80	41	39	South	Clear, and little wind.
	19	88	36	40	Ditto	Clear, moderate wind.
♂ 13	1	93	53	45	S. E.	Very hazy.
	4	90	50	42	Eaſt	Ditto, the wind briſk.
	9	87	44	40	Ditto	Cloudy, and ditto.
	19	79	35	37	Ditto	Ditto.
☿ 14	2	51	38	40	North	Ditto, the wind ſtrong.
	9	34	42	40	Eaſt	Rain and ſtrong wind.
	20	26	45	60	S. W.	Very hazy, ditto.
♃ 15 *	0				Ditto	Rain, ditto.
	10	37	48	46	Ditto	Cloudy, and moderate wind.
	20	28	40	41	Weſt	Rain and ſtrong wind.
♀ 16	0	28	40	43	Ditto	Cloudy and ditto.
	9	40	40	36	Ditto	Slow rain, and little wind.
	20	38	36	38	N. W.	Cloudy, and moderate wind.
♄ 17	2	39	41	46	N. W.	Ditto.
	10	40	38	38	Ditto	Ditto.
						——— J. D.

* N. B. The 15th, the ice of the river broke and went to ſea. J. D.

1769.	Ho.	Baro.	Thermome. A	Thermome. B	Winds	Weather, &c.
♄ June 17, at	19	29 42	+37	+39	N. W.	Cloudy, with ſmall rain, wind briſk.
☉ 18	0	47	42	50	Ditto	Cloudy, the wind moderate.
	6	57	44	44	Ditto	Ditto.
	12	54	38	35	Ditto	Thick clouds, the wind high.
	19	61	36	42	Ditto	Cloudy, the wind briſk.
☽ 19	0	69	48	54	Ditto	Ditto.
	6	73	51	55	Ditto	Ditto, little wind.
	19	75	43	47	Ditto	Clear, wind moderate.
♂ 20	0	76	57	64	Eaſt	Ditto, and very little wind.
	3			70		
	6	71	61	65	S. E.	Cloudy, and little wind.
	19	51	47	49	Ditto	Thick and rainy, the wind ſtrong.
☿ 21	0	44	50	63	South	Flying clouds, the wind as above.
	6	47	59	57	Eaſt	Cloudy, and little wind.
	19	74	40	41	N. W.	Clear, ditto.
♃ 22	0	79	54	56	Eaſt	Hazy, and very little wind.
	19	68	46	49	Ditto	Ditto.
♀ 23	0	65	58	58	Ditto	Ditto.
	6	56	60	68	S. E.	Ditto, more wind.
	19	50	47	45	North	Cloudy, wind briſk.
♄ 24	0	56	53	49	Ditto	Very foggy, and little wind.
	6	59	57	57	Eaſt	Clear, ditto.
						W. W.
	20	29 53	+53	+61	South	Clear, the wind briſk.
☉ 25	0	50	63	74	S. W.	Ditto.
	4	50	69	78	Ditto	A thunder ſhower, the wind moderate.
	9	60	55	43	Calm	Cloudy.

☉ June

1769.	Ho.	Baro.	Thermome. A	Thermome. B	Winds.	Weather, &c.
☉ June 25, at	20	29 68	+45	+49	Eaſt	Cloudy, and moderate wind.
☽ 26	1	73	50	54	N. E.	Ditto.
	9	74	49	44	Ditto	Ditto.
	20	73	45	55	S. E.	Very hazy, and little wind.
♂ 27	1	75	54	60	Eaſt	Cloudy, and moderate wind.
	9	72	54	50	South	Rain, ditto.
	20	69	49	55	Calm	Very hazy.
☿ 28	0	73	58	59	Eaſt	Clear, and little wind.
	9	80	60	55	Ditto	Ditto.
	20	81	50	52	S. W.	Cloudy, and ditto.
♃ 29	1	83	52	63	Weſt	Clear, and little wind.
	9	91	60	56	Calm	Very hazy.
	20	91	55	58	North	Very thick clouds, and little wind.
♀ 30	2	89	57	60	Eaſt'ly	Cloudy, and ditto.
	8	84	52	49	Ditto	Rain, and ditto.
	20	76	45	44	North	Ditto.
♄ July 1	9	76	46	44	Ditto	Cloudy, and moderate wind.
						J. D.
	19	29 74	+44	+48	N. E.	Cloudy, the wind moderate.
☉ 2	1	74	57	60	North	Flying clouds, and very little wind.
	6	73	66	64	Ditto	Ditto.
	19	58	51	54	South	Ditto, the wind briſk.
☽ 3	0	52	61	76½	Ditto	Ditto.
	3			80	Ditto	Ditto.
	6	48	69	74	Ditto	Ditto.
	19	41	56	61	S. E.	Very hazy, and little wind.
♂ 4	0	40	67	75	Eaſt	Cloudy, ditto.
	6	42	58	49	North	Very thick and foggy, wind high.
	20	50	47	47	Ditto	Ditto.
☿ 5	0	56	47	51	Ditto	Ditto.

1769.	Ho.	Baro.	Thermome. A	B	Winds.	Weather, &c.
☿ July 5, at	8	29 66	+48	+45	N. W.	Cloudy, the wind moderate.
	19	79	44	46	North	Ditto, very little wind.
♃ 6	0	87	47	54	Ditto	Ditto.
	7	96	49	46	N. W.	Ditto.
	19	30 08	45	49	North	Very hazy, the wind moderate.
♀ 7	0	13	53	55	Ditto	Cloudy, ditto.
	6	16	55	53	Ditto	Thin clouds, ditto.
	19	14	46	50	N. E.	Ditto.
♄ 8	0	15	54	58	North	Cloudy, ditto.
	6	15	54	48	N. E.	Thin clouds, ditto.
						——W. W.
	19	30 13	+44	+46	N. E.	Clear.
☉ 9	0	16	53	51	North	Ditto.
	9	08	54	50	S. E.	Ditto.
	20	29 92	50	61	S. W.	Ditto.
☽ 10	0	84	61	79	Ditto	Very hazy.
	10	91	52	46	N. E.	Cloudy.
	21	92	46	52	Calm	Ditto.
♂ 11	9	86	54	53	Eaſt	Clear.
	20	61	45	55	South	Cloudy.
☿ 12	2	46	65	85	S. W.	Hazy, and briſk wind.
	9	51	60	48	North	Cloudy.
	20	55	49	52	Ditto	Hazy.
♃ 13	4	59	59	62	South	Very hazy.
	9	56	56	52		Cloudy, and very little wind.
	20	73	48	48	North	Clear, and ſtrong wind.
♀ 14	0	73	55	55	Weſt	Ditto, the wind moderate.
	9	64	55	55	Ditto	Rain.
	20	60	49	48	North	Clear.
♄ 15	0	64	54	55	North	Flying clouds.
	9	65	55	50	South	Clear, and moderate wind.
						——J. D.

1769.	Ho.	Baro.	Thermome. A	B	Winds.	Weather, &c.
♄ July 15, at	19	29 64	+ 46	+49	South	Clear, and the wind moderate.
☉ 16	0	62	59	69	Ditto	Very hazy, and little wind.
	6	58	62	56	N. E.	Thick, the wind moderate.
	19	45	50	54	North	Thin clouds, and little wind.
☽ 17	0	51	60	60	Ditto	Clear, ditto.
	8	52	61	56	Calm	Cloudy.
	19	50	53	54	N. W.	Flying clouds, the wind briſk.
♂ 18	0	48	61	66	North	Thick clouds, and very little wind.
	6	44	59	62	Weſt	Cloudy, and little wind.
	19	51	51	52	North	Cloudy, the wind briſk.
☿ 19	0	56	57	64	N. W.	Ditto, and little wind.
	6	56	60	61	Ditto	Ditto, and very little wind.
	19	53	50	49	Ditto	Thick ditto.
♃ 20	0	57	54	56	N. E.	Hazy, the wind moderate.
	6	6	59	58	Ditto	Clear, and little wind.
	19	61	46	51	South	Very thick, ditto.
♀ 21	0	60	50	58	North	Flying clouds, ditto.
	6	66	58	62	Ditto	Very hazy, ditto.
	11				South	Cloudy, with large flaſhes of lightening.
	19	67	48	54	N. W.	Ditto, and little wind.
♄ 22	0	62	51	58	Ditto	Rain, ditto.
	6	64	58	60	North	Flying clouds, ditto.
	12	61	52	49	N. W.	Cloudy, briſk wind.

W.W

	19	29 60	+49	+49	Weſt	Clear, ſtrong wind and frequent ſhowers.

♄ July

1769.	Ho.	Baro.	Thermome.		Winds.	Weather, &c.
			A	B		
☉ July 23, at	1	29 60	+55	+57	Weſt	Showers, and ſtrong wind.
	8	61	55	54	Ditto	Ditto, and moderate wind.
	20	63	49	51	N. W.	Clear.
☽ 24	0	67	62	64		Rain, and very little wind.
	4					Hazy.
	8	66	63	59	S. E.	Cloudy.
	19	64	52	56		Clear.
♂ 25	0	62	62	71	South	Very hazy.
	4					Rain, and ſtrong wind.
	9	61	61	58	Weſt	Cloudy, and little wind.
	19	60	51	55	South	Hazy, and little wind.
☿ 26	0	52	65	80	S. W.	Very hazy.
	9	54	54	64	Eaſt	Some very black clouds and thunder to the ſouthward.
	19	55	54	55	Ditto	Hazy, and little wind.
♃ 27	0	53	60	59	N. E.	Ditto, and moderate wind.
	19	60	49	53	N. W.	Hazy, and wind briſk.
♀ 28	0	64	58	63	Weſt	Hazy, briſk wind.
	9	66	63	60	S. W.	Thick haze, and little wind.
	19	65	53	54	Weſt	Clear.
♄ 29	0	69	64	66	Ditto	Cloudy
	4					Ditto.
	9	69	62	59	Weſt	Thin clouds, and little wind.
						J. D.
	19	29 64	+52	+58	Weſt	Cloudy, and little wind.
● 30	0	65	61	65	Eaſt	Flying clouds, ditto.
	6	65	67	65	Ditto	Clear, ditto.
	20	64	53	54	N. W.	Thin clouds, the wind briſk.
☽ 31	0	69	58	59	North	Ditto, ditto.

☽ July

1769.	Ho.	Baro.	Thermome. A	B	Wind.	Weather, &c.
☽ July 31, a	6	29 71	+57	+53	N. W.	Cloudy, little wind.
	19	71	50	51	Ditto	Thick, the wind moderate.
♂ Aug. 1	0	72	56	63	Ditto	Cloudy, and little wind.
	6	73	63	61	N. E.	Ditto.
	19	70	51	53	Ditto	Ditto.
☿ 2	0	73	57	59	Ditto	Thin clouds, and little wind.
	6	74	59	60	Calm	Cloudy.
	19	69	54	56	Weſt	Ditto, and very little wind.
♃ 3	0	68	55	50	North	Rain, and moderate wind.
	6	67	54	54	N. E.	Clear, and little wind.
	19	65	44	48	Calm	Cloudy.
♀ 4	0	64	56	55	Eaſt	Clear, and very little wind.
	6	64	58	57	Ditto	Ditto, the wind moderate.
	19	60	45	46	Ditto	Thick clouds, and very little wind.
♄ 5	0	67	55	54	Ditto	Clear, ditto.
	6	72	58	57	Ditto	Ditto, moderate wind.
						W. W.
	19	29 83	+53	53	S. E.	Cloudy, wind briſk.
☉ 6	0	86	56	68	South	Ditto.
	7	81	57	54	S. E.	Ditto.
	19	43	49	52	Weſt	Cloudy: has been much rain, and ſtrong wind.
☽ 7	1	49	51	55	Ditto	Cloudy, and ſtrong wind.
	8	53	54	54		Thin clouds, and little wind.
	19	50	48	50	Weſt	Clear, and briſk wind.
♂	0	49	54	58	Ditto	Cloudy, and ſtrong wind.

♂ Aug.

1769.		Ho.	Baro.	Thermome. A	B	Winds	Weather, &c.
♂	Aug. 8, at	8	29 46	+55	+51	Weſt	Showers, and ſtrong wind.
		20	41	49	51	Ditto	Moſtly cloudy, and ditto.
☿	9	0	39	52	60	Ditto	Cloudy, and briſk wind.
		8	38	54	52	Ditto	Very hazy, and briſk wind.
		20	22	51	58	Ditto	Cloudy, ditto.
♃	10	1	21	55	60	Ditto	Flying clouds, and ſtrong wind.
		8	29	55	55	Ditto	Ditto, ditto.
		19	39	49	52	N.W.	Cloudy, ditto.
♀	11	0	51	55	61	Ditto	Ditto, leſs wind.
		8	56	58		Ditto	Flying clouds, and moderate wind.
		19	60	50	51	North	Thick clouds, and briſk wind.
♄	12	0	63	56	61	Ditto	Flying clouds, and ditto.
		8	73	57	53	Calm	Cloudy.
							J. D.
		19	29 73	+51	+52	N.W.	Cloudy, and little wind.
☉	13	1	76	57	65	N. E.	Ditto, ditto.
		7	78	60	60	N. E.	Ditto.
		18	78	49	49	Ditto	Ditto.
☽	14	4	82	53	52	Ditto	Ditto.
		19	90	47	47	Ditto	Ditto.
♂	15	0	92	49	56	Ditto	Ditto.
		7	92	52	57	Calm	Flying clouds.
		19	98	46	48	Eaſt	Clear, and little wind.
☿	16	0	30 00	55	58	Ditto	Ditto.
		8	29 93	51	48	Ditto	Clear, and briſk wind.
		19	83	44	48	Ditto	Very hazy, ditto.
♃	7	0				Ditto	Very thick, with heavy rain.
		2	76	48	55	Ditto	Very thick, the wind high.

♃ Aug.

1769.	Ho.	Baro.	Thermome. A	B	Winds.	Weather, &c.
♃ Aug. 17, at	6	29 62	+50	+50	Eaſt	A very thick fog, with drizling rain, the wind ſtrong.
	20	30	46	45	N. E.	Much rain, and the wind ſtrong.
♀ 18	0	27	46	49	Ditto	Ditto, ditto.
	6	26	46	43	North	Ditto.
	19	30	43	44	Ditto	Very thick, ditto.
♄ 19.	0	36	44	47	Ditto	Ditto, ditto.
	6	40	42	44	Ditto	Ditto.
						——— —W. W.
	19	29 63	+43	+44	North.	Strong wind, and cloudy.
☉ 20	2	75	45	49	Ditto	Strong wind; but ſomething leſs; and clear.
	7	80	44	46	Ditto	Cloudy, and ſtrong wind.
	19	80	44	48	Ditto	Thin clouds, and briſk wind.
☽ 21	0	80	53	57	S. W.	Cloudy, and moderate wind.
	20	50	47	51	South.	Rain, and moderate wind.
♂ 22	0	48	54	59	S. W.	Clear, ditto.
	8	50	59	53	South	Ditto.
	19	50	50	50	Ditto	Clear, very little wind.
☿ 23	0	50	60	60	S. E.	Flying clouds, ditto.
	19	44	49	48	Weſt	Very thick, and little wind.
♃ 24	5	55	55	51	North	Clear.
	20	53	45	48	N. W.	Foggy.
♀ 25	0	61	51	52	North	Clear.
	4	64	58	57	Ditto	Moſtly cloudy.
	20	59	47	54	–	Cloudy, and very little wind.
♄ 26	6	63	59	56	N. E.	Cloudy, and moderate wind.
						——— J. D.

			Thermome.			
1769.	Ho.	Baro.	A	B	Winds.	Weather, &c.
♄ Aug. 26, at	19	29 69	+47	+49	North	Cloudy, the wind moderate.
☉ 27	0	70	52	53	Ditto	Very hazy, and briſk wind.
	7	73	55	49	Ditto	Cloudy, ditto.
	19	78	46	44	N. E.	Ditto. Took down the inſtruments, and packed them up.
						—W. W.

N. B. The thermometer A was within the lower obſervatory, on the ſouth-eaſt ſide, cloſe to the barometer; and its ball was exactly the ſame height with the mercury in the baſon thereof. The thermometer marked B was without doors, on the north ſide of the obſervatory.

The ſigns + and — which are annexed to the altitudes of the two thermometers ſignify, that they ſtood ſo many degrees above or below the cypher reſpectively. Both the thermometers are according to Fahrenheit's ſcale. W. W.

XV. *An Account of ſome very perfect and uncommon Specimens of* Spongiæ *from the Coaſt of* Italy: *In a Letter to* James Weſt, *Eſquire, Preſident of the* Royal Society; *from* John Strange, *Eſquire, F. R. S.*

DEAR SIR,

Read March 22, 1770. HAVING had frequent opportunities, during my ſtay in Italy, of viſiting the ſea coaſts, I was thereby encouraged, among other reſearches after the antiquities and natural hiſtory of that country, to collect ſome ſpecimens of ſubmarine productions. Upon examining the ſouth-weſt coaſt of Italy in particular, I happened to meet with ſome very perfect and curious ſpecimens of ſpongiæ, the deſcriptions of which I now ſend you. One of theſe (1) has never been deſcribed before; and ſince only fragments of the other two ſpecimens have been delineated in the works of the authors who mention them (2), I have not ſcrupled to add likewiſe their reſpective deſcriptions and figures.

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(1) Plate VI. Fig. 3, 4.

(2) Imperat. Stor. Nat. Lib. XXVII. Cap. 8. Fig. 7. Marſigli Hiſt. Phyſ. de la Mer. Pl. 16. Fig. 79.

Theſe deſcriptions may perhaps appear imperfect, being confined merely to the figure and ſubſtance of the bodies, without any mention of the polypes that inhabited them. To account for this omiſſion, it is neceſſary to acquaint you, that they were drawn up a few years ago, with others of the like kind, at the requeſt of Doctor Targioni Tozzetti of Florence, who deſigned them for an appendix to a poſthumous work of Micheli's, entituled, *De Plantis Marinis*. As the plan of this work was botanical, I therefore thought it neceſſary to accommodate my deſcriptions accordingly, though I was not inclined to his opinion about the origin of theſe bodies. Upon enquiry it appears, that the publication of Micheli's work is very uncertain; for which reaſon I ſend you the following deſcriptions, requeſting at the ſame time, your communication of them to the gentlemen of the Royal Society, ſhould you think they deſerve their notice. It is hoped, that they will not prove altogether unacceptable, as the Zoologiſts, in their deſcriptions of the Polypes, are ſometimes apt to paſs over too ſlightly the figure and ſtructure of the parts in which they reſide.

DESCRIPTIONES.

1. Spongia ſtuppea, tuberoſa et calamifera, calamis cylindraceo-conoideis, in ceſpitem connatis, omnibus aſſurgentibus, quorum nonnulli majores in apice, calicis in modum, pyxidati viſuntur, baſi bifida et trifida, binis vel tribus columellis inſtructa; calamis minoribus myoſiroideis, ſive caudiculæ muris formam præ ſe ferentibus, et in rotunde acutum terminatis.

Fig 3.

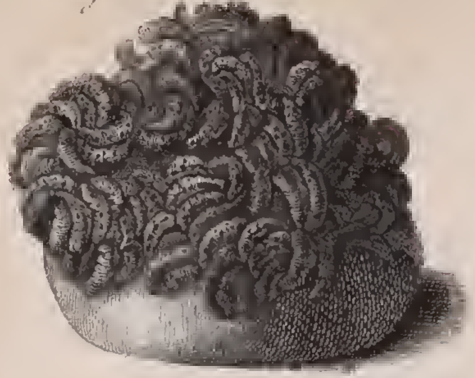


Fig 4.

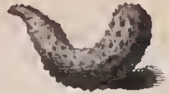


Fig 2

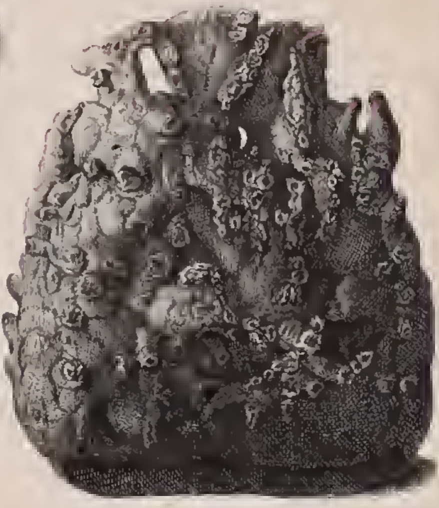
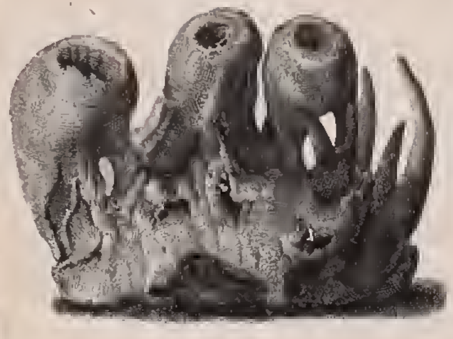


Fig 1



James Basire Sculp

minatis. Corpus conſtat materia parum coactili; colore atrofuligineo. Invenitur in littore Etruſco non longe a Populonia, loco ab incolis dicto *Porto Baratto*, at non adeo communis; vidi frequenter ad littus Neapolitanum. Tab. VI. Fig. 1. Dantur varietates quam plurimæ, calamis quaſi perfecte conoideis, erectis, obliquis, ſibi invicem inverſis, baſi integra, nonnunquam foraminibus pervia.

2. Spongia ſtuppea, tuberculoſa, tuberculis ſimplicibus et ramoſis, ramis vero imperfectis et plerumque ad radices obtruncatis, forma corporis ſubconoidea, in apice complanata, ſuperficie tota inæquali, ubique ramuſculis et tuberibus horrida, baſi concavata et bifida. Corpus conſtat materia parum coactili, interne ſatis anfractuoſa, cavitatibus nonnullis latioribus, ſeu magis excavatis; colore corporis fuligineo. Invenitur ad littus Etruſcum, qua Populoniam ab oriente hyemali ſpectat, haud procul ab oſtio fluminis ab incolis dicti *la Cornia*. Hæc ſpongiæ ſpecies e baſi lata et excavata aſſurgens, in ſummitatem fere ſemper complanatam et ſubconoideam definit, mitræ formam non male referens; immo a piſcatoribus littoris Neapolitani, ubi ſatis abundat, vocabulo vulgari *Mitra Papale* nuncupatur, prout a viro fide digno jampridem accepi. Tab. VI. Fig. 2. An

> Alcyonium durum, Preſbyterorum pileolum prorſus effingens. Cupani Hort. Cathol. Suppl. 1.?

3. Spongia minima, forma corporis ſubconoideo inverſa, et vermiculi in modum contorta. Materia conſtat denſiſſima et parum coactili, fibris arcte inter

ſe cohærentibus, prout in hircina notatur; colore corporis obſcure fuſco. Gregatim inveniuntur, ſaxis, teſtis conchyliorum, variiſque aliis corporibus marinis arcte cohærentes. Rariſſima eſt hæc ſpongiæ ſpecies, nec a nemine, quod ſciam, adhuc deſcripta vel adumbrata. Marſilius comes analogum quid dudum deſcripſit ſub nomine *Eperon de coq* (3); a noſtra vero ſatis differt tam colore quam capite pyxidato, prout in figura viſitur. Spongiæ noſtræ fruſtula nonnulla ſolitaria inveni in littore Etruſco inter Populoniam atque oſtium fluminis Cecinæ. Exemplar integerrimum unicum vidi e mari Etruſco expiſcatum penes dominum Philippum Fabrini et in muſeolo ſuo Piſis probe aſſervatum. Exhibet congeriem ejuſdem ſpongiæ dorſo lapidis calcarei adnatam, cujus icon in Tabula VI. exhibetur. Fig. 3. 4.

It is well known, from the obſervations of Mercati, Boccone, Donati, and others, that the coaſts of Italy in general afford a remarkable variety of zoophytes. Pallas (4) likewiſe particularly mentions the many ſpecies of gorgoniæ found on that coaſt, and juſtly laments the indolence of the Italians in not regarding them. Mathioli, Mercati, Ferrante Imperato, and the other early writers in natural hiſtory, made few, if any, new obſervations of this kind. They did little more than copy the ancients, or one another; and thought it ſufficient to aſcertain the identity of the ſpecies deſcribed by Ariſtotle, Dioſcorides, and Pliny, and to illuſtrate them by figures,

(3) Hiſt. Phyſ. de la Mer, part 4. pag. 63. pl. 5. n. 22.
(4) Zooph. ad Gen. Gorgon.

which were wanted in the works of theſe old maſters. Thus, for inſtance, Mathioli and Ferrante Imperato deſcribe only the Alcyonia of Dioſcorides. The more modern Italian naturaliſts have made as little progreſs in this ſubject, from the influence of the opinion eſtabliſhed amongſt them by Micheli and Marſigli; for, if we except Donati, ſcarce any of the reſt have embraced the preſent received ſyſtem about the origin of Zoophytes, though the diſcoveries of the French academiſts, added to thoſe of Ellis, Pallas, and other ingenious writers, ſeem to have put this matter beyond a doubt. But it is time to relieve you from this ſubject, and to ſubſcribe myſelf, with great regard,

Dear Sir,

Your moſt obedient

humble ſervant,

Cavendiſh-Square,
Nov. 16th, 1769.

John Strange.

XVI. *A Letter from Captain* Davies *to* John Ellis, *Eſquire*, F. R. S. *on a Method of preparing Birds for Preſervation.*

SIR,

Shooter's-Hill, 12th March, 1770.

Read March 22, 1770.

AS officers in his Majeſty's army and navy have greatly the advantage in general, of moſt people (by means of their profeſſion) of viſiting different parts of the world, they have of courſe more frequent opportunities of meeting with many new and wonderful productions; which, though they do not paſs altogether unheeded, are in a manner loſt to mankind, for want of knowledge in the diſcoverer, of modes for preſerving ſuch things as may appear extraordinary or worth adding to the collections of the curious. I have therefore, for the advantage of the gentlemen of thoſe profeſſions, or any others, diſpoſed to enlarge the funds of natural hiſtory already extant, (knowing no one labours more aſſiduouſly to add to the knowledge of the productions of nature than yourſelf), taken the liberty of ſending you an account of a proceſs for the preſervation of birds, beaſts, and other extraordinary productions of the animal world, which I have by a ſeries of experiments, and variety of methods and compoſitions, found to anſwer better than any I have ever met with. A ſpecimen of my own preſervation I have alſo ſent with this, in order for your inſpection; and if you ſhould think worth while, to communicate the ſame to the Royal Society, as by that means it may be publiſhed to the world, for the advantage of thoſe who are deſirous of adding freſh ſpecimens to the collections of natural curioſities.

The RECEIPT is as follows:

Let a bird, beaſt, or any ſuch like production of nature, be procured, that has been well preſerved in its death, either naturally or by ſhot, as thoſe that intend making any tolerable collection muſt do. I would not recommend ſhooting them (birds in particular) with ſhot ſmaller than common partridge ſhot, or N°. 5, and that at a conſiderable diſtance, to prevent their being torn with too great a number. Having procured a bird as aforeſaid, let it be opened from the upper part of the breaſt, to the vent, with a ſharp knife or pair of ſciſſars, the feathers of the breaſt and belly being firſt carefully laid aſide by the fingers, ſo as not to hinder the ſkin being eaſily come at. The ſkin muſt then be carefully looſened from all the fleſhy parts of the breaſt, body, thighs, and wings; then cut off all the fleſh from thoſe parts, and take out alſo the entrails and all the inſide: then, having got a compoſition of burnt allum, camphire, and cinnamon, of each an equal quantity, well powdered and mixt together, ſtrew ſome of this powder lightly over the whole carcaſe, but ſalt is by no means to be uſed with this compoſition, as it always will drop and naſty the plumage in moiſt weather; pour alſo into the body a ſmall quantity of camphire diſſolved in rectified ſpirits of wine; after that, fill up the cavity with fine cotton, or any ſoft woolly ſubſtance, pouring ſome of the aforeſaid ſpirits into the cotton, or ſtuffing. Open next the mouth, and with a pair of ſciſſars take away the tongue, the roof of the mouth, eyes, brains, and inſide of the head; fill that alſo with the ſame compoſition; and having procured eyes as near the natural ones as poſſible, put them into the ſockets by means of a ſmall pair of nippers introduced at the mouth. The eyes will be beſt made by dropping drops of black ſealing wax on a card of the ſize of the natural ones; the card muſt be cut something larger than the wax to prevent their falling out of the head. Fill the head quite full with cotton, pouring ſome of the ſpirits down the throat, with ſome of the powder; a ſmall piece of braſs wire, that has been heated in the fire to make it pliable, may be put down the throat, being paſſed through one of the noſtrils, and faſtened to the breaſt bone, to place the head in any attitude you chooſe; next fill up the body where the fleſh has been taken away, with cotton and your compoſition; and, having a fine needle and ſilk, ſew up the ſkin, beginning at the breaſt, obſerving, as you approach towards the vent, to ſtuff the ſkin as tight as it will bear. This will be eaſieſt accompliſhed by means of a ſmall piece of ſtick or ivory, like a ſkewer, till the whole is done: then lay your feathers of the breaſt and belly in their proper order, and your bird will be compleated;

compleated. If you would chuſe to put it into an attitude, by introducing a ſmall piece of the wire above-mentioned through the ſole of each foot up the leg, and into the pinion of each wing, it may be diſpoſed of as you pleaſe.

A compoſition of ſublimate mercury, tempered with ſome water, and rubbed gently over the feathers, will prevent inſects, and other vermin, from deſtroying the plumage; but, as you have treated already very ſufficiently on the ſublimate compoſition, I ſhall conclude, with aſſuring you, I am,

SIR,

With great eſteem,

your moſt obedient and

obliged humble ſervant,

Tho. Davies,

Captain Lieutenant of Artillery.

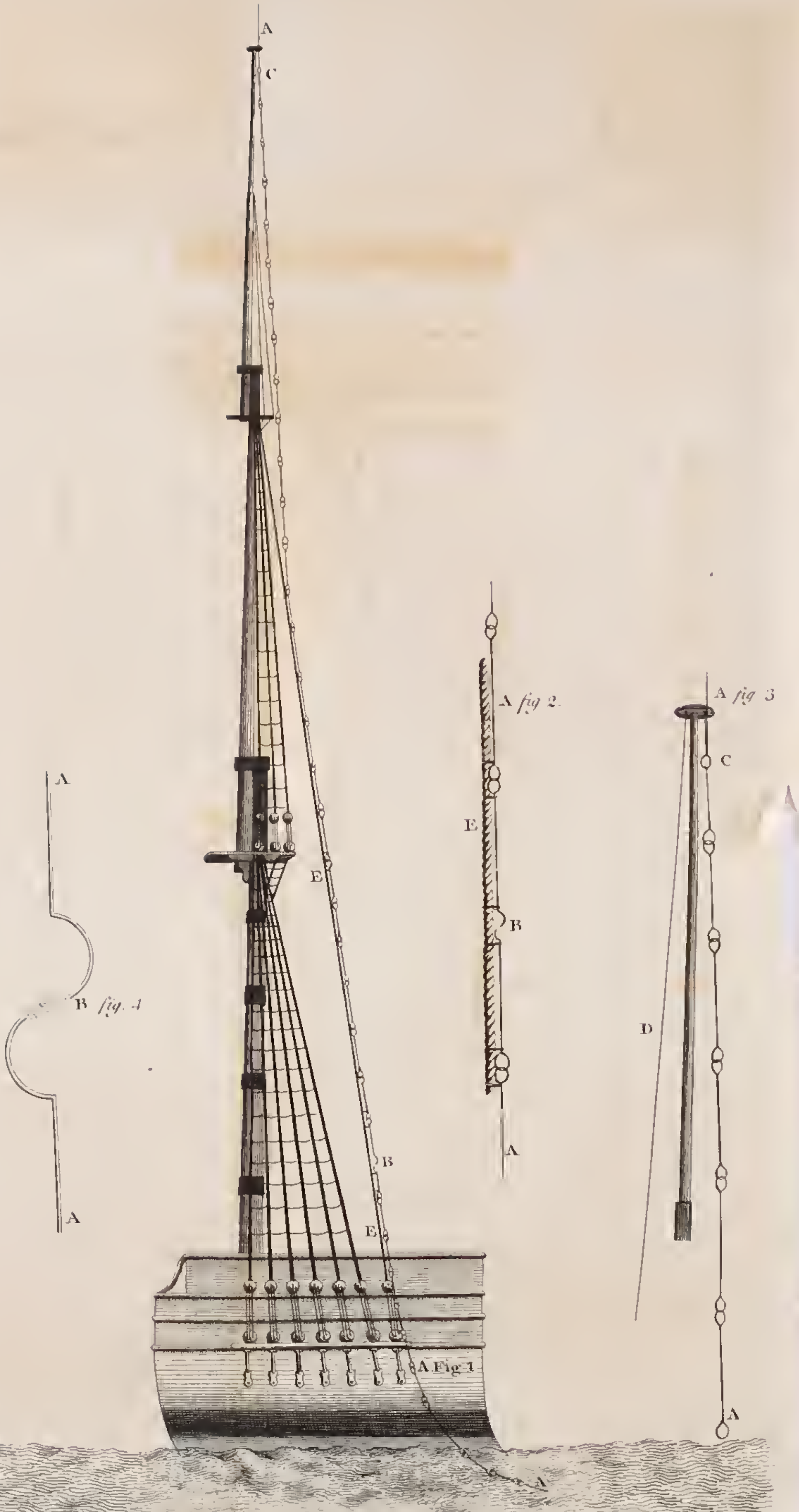
Rtceived February 1, 1770,

XVII. *A Letter to Dr.* Benjamin Franklin, *F. R. S. giving an Account of the Appearance of Lightning on a Conductor fixed from the Summit of the Mainmaſt of a Ship, down to the Water: By Capt.* J. L. Winn.

S I R,

Read March 29, 1770.

IT is a common, and, I am afraid, juſt complaint, that ſeamen are exceedingly backward in availing themſelves of the diſcoveries which men of ſcience have made, and the directions which they have given for their benefit and ſafety. Notwithſtanding the pains ſeveral eminent philoſophers have taken, to bring conductors into general uſe, as well in ſhips as houſes; it is too true that very few veſſels are furniſhed with them, though ſcarce a year paſſes, that does not afford us inſtances (ſome of them terrible ones), of ſhips being ſtruck by lightening: for my part, I am never without a conductor in my ſhip. I have had them of various conſtructions: that which I now uſe, is a chain of copper wire, as deſcribed in the annexed plate. That ſuch a chain, ſo diſpoſed, may conduct the lightening, and prevent a ſtroke that might deſtroy a ſhip, has often been demonſtrated; but a circumſtance



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ſtance that occurred in my laſt voyage, may perhaps have greater weight with ſome ſeamen, than all the reaſonings of the electricians. If it ſhould be a means of perſuading them to make uſe of conductors, my intention will be anſwered.

In April laſt, as we approached the coaſt of America, we met with ſtrong South-weſterly gales: they had continued ſeveral days, when exceeding dark heavy clouds aroſe in the oppoſite quarter, forced againſt the wind that blew with us, till they had covered all the North-eaſtern half of the hemiſphere: the ſtruggle then between the two winds was very extraordinary; ſometimes one prevailing, ſometimes the other. I was apprehenſive we ſhould have much lightning, and got my conductor in order; when, in hauling up the mainſail, the ſheet block ſtruck violently againſt the back-ſtays, to which the chain was faſtened (E E in fig. 1. TAB. VII.), and, as I found afterwards, broke the latter, which occaſioned the phænomenon I am going to deſcribe. It was near midnight and very dark, when I firſt obſerve a pale bluiſh light a few feet above the quarter rail: at firſt I thought it proceeded from the light in the binnacle; but, finding that it frequently diſappeared and returned again preciſely in the ſame place, and that it ſometimes emitted ſparks not unlike thoſe of a ſmall ſquib, I began to ſuſpect that it proceeded from the conductor. To be certain, I ordered all the lights to be put out below, and, that no rays of light might iſſue from the binnacle, I covered it entirely with my cloak. I was preſently confirmed in my conjecture, that the light and ſparks which I had obſerved proceeded from the chain; for, placing myſelf near it, during the ſpace of two hours and a half, I ſaw it frequently emit continued ſtreams of rays or ſparks (ſee fig. 4.); ſometimes ſingle drops as it were ſlowly ſucceeding to each other, and ſometimes only a pale feeble light. On examining next morning, I found the chain broken at B, half the eye of each link being quite gone, and the points of the remaining halves about three fourths of an inch aſunder: luckily, the chain was faſtened to a ſmaller rope (fig. 2.) above and below the eye of each link, which prevented that part of the chain below B, from falling into the water, or of being ſeparated from the part above B, beyond the ſtriking or attracting diſtance.

I am with the greateſt reſpect,

SIR,

Your obliged humble ſervant,

J. L. Winn.

References to PLATE VII.

AAA The conductor, a chain of copper wire, of the thickneſs of the barrel of a ſmall quill, the uppermoſt and loweſt links of which terminate in fine points.

B Fig. 1. The broken links; in paſſing between which, the lightning became viſible.

C An eye in the uppermoſt link, to which the pendant halliards are faſtened.

D Fig. 3. The pendant halliards, which paſs over a ſheave in the truck, on the top-gallant-maſt-head, and are faſtened to the eye at C, and hoiſt up the chain, till the point of the uppermoſt link is a foot or two above the truck.

EE The maintop-maſt back-ſtay, to which the chain is ſtopped, to prevent its ſwinging about.

B Fig. 4. Appearance of the lightning paſſing between the broken limbs.

Received February 22, 1770.

XVIII. *An Inveſtigation of the lateral Exploſion, and of the Electricity communicated to the electrical Circuit, in a Diſcharge: By* Joſeph Prieſtley, *LL. D. F. R. S.*

Read March 29, 1770.

SEVERAL years before I made any experiments in electricity, except with a view to amuſe myſelf, and my friends; I had obſerved, that in diſcharging jars, and particularly ſuch as were filled with water, without any coating on the out ſide, I felt a ſlight ſhock; though it was plain that the hand, in which I held the diſcharging rod, made no part of the circuit.

Mr. Wilſon, alſo, in his firſt experiments on the Leyden phial, obſerved, that bodies placed without the electric circuit would be affected with the ſhock, if they were only in contact with any part of it, or very near it. Analogous to this was his obſervation, that, if the circuit was not made of metals, or other very good conductors; the perſon who laid hold of them, in order to perform the experiment, felt a conſiderable ſhock, in that arm which was in contact with the circuit. See Hiſtory of Electricity, p. 95.

Laſtly, in the courſe of my experiments with large electrical batteries, I found the force of this lateral exploſion (as I ſhall chuſe to call it) to be very conſiderable: for I ſeveral times obſerved, that a chain which communicated with the outſide of the battery, but which made no part of the circuit, made a black ſtain on a piece of white paper, on which it accidentally lay, almoſt as deep as the chain that formed the circuit. (Hiſtory, p. 644.) And when, in order to judge, by my feeling, of the lateral force of electrical exploſions; I made it paſs over a part of my naked arm, the hairs of the ſkin were all ſinged, and the *papillæ pyramidales* raiſed, not only along the path of the exploſion, but alſo wherever any part of the chain had touched it, though not in the circuit. Ib. p. 686.

It was to aſcertain the nature and effects of this lateral exploſion, that the following experiments were made; and in reciting them, I ſhall diſtinctly note the progreſs of my own thoughts in the courſe of the inveſtigation, from a ſtate of abſolute uncertainty, to that of the fulleſt ſatisfaction; and, I flatter myſelf, that ſome of the facts I ſhall exhibit, will give ſurprize and pleaſure to thoſe, who are beſt acquainted with, and moſt intereſted in, the hiſtory of electricity.

Not having the leaſt doubt, but that if any electric ſpark paſſed between a body that was inſulated, and another, the inſulated body would appear, either to have received, or to have loſt, electricity; I imagined that nothing more was to be done, than to inſulate bodies placed within the influence of the electric circuit, with pith balls hanging from them; and, upon their diverging with the electric ſpark, immediately to obſerve, of what kind the electricity they had contracted was; and previous to the experiment, I conjectured it would be negative; ſuppoſing that the diſcharge from the inſide coating, in an interrupted circuit, was not able to ſupply the outſide faſt enough. And ſince, the larger the inſulated body was, the greater quantity of the electric fluid it was capable of receiving, or parting with, and conſequently the more ſenſible the effect would be; I began with ſuſpending on ſilken ſtrings, a paſteboard tube, covered with tinfoil, ſeven feet long and four inches thick, with large knobs at each end; and a braſs ball (at the end of an iron rod, which communicated with the outſide of the jar) was placed within about a quarter of an inch of it; while the diſcharge was made through an inſulated interrupted circuit, no part of which was leſs than two feet from the inſulated tube. On making the exploſion, the ſpark appeared as I expected; but, to my great ſurprize, I could not find that either poſitive or negative electricity was communicated to the inſulated tube. Neither the pith balls, nor the fineſt threads diverged, or moved in the leaſt, at or after the diſcharge; though, every thing elſe remaining in the ſame ſtate, the leaſt ſenſible electricity communicated to this tube (a quantity ſo ſmall as hardly to be viſible, in the form of a ſpark, at the time of communication) made the balls and the threads ſeparate to a great diſtance, and would have kept them in a ſtate of divergency more than an hour. Leſt a ſmall degree of motion or divergency ſhould eſcape my notice, while I was intent upon making the diſcharge, I had an aſſiſtant along with me, whoſe eye was upon the threads all the time I was making the experiment.

This experiment, as will eaſily be imagined, ſhook my whole hypotheſis, and confounded all my ideas. I could not queſtion the fact, having repeated the experiment, with preciſely the ſame event, I believe, above fifty times, on account of having been hardly able to believe my own ſenſes, or thoſe of others. There was an evident electric ſpark, ſometimes near half an inch in length, betwixt the bodies compoſing the electric circuit and the inſulated tube, in ſuch a ſtate of the air, as I knew, by frequent trials, would have kept it electrified a long time, and yet there was no communication of electricity.

I do not remember that I was ever more puzzled with any appearance in nature than I was with this; and, in the night following theſe experiments, endleſs were the ſchemes that occurred to me, of accounting for them, and the methods with which I propoſed to diverſify them the next morning, in order to find out the cauſe of this ſtrange phænomenon. Accordingly, I was no ſooner at liberty to attend to this experiment; but, repeating it with ſome difference in the diſpoſition of the apparatus, I obſerved that, upon every diſcharge, a ſlight motion was given to the threads that hung from the inſulated tube. Upon this the impoſſibility of an electric ſpark, neither giving nor taking any thing from an inſulated body, contrary to my moſt attentive obſervation, and that of my aſſiſtants, I concluded that ſome motion muſt have been given to the threads the day before; eſpecially when I found that, in theſe latter experiments,

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the communicated electricity was always poſitive, the ſame with that of the inſide of the jar; but the quantity of it was ſo ſmall, that the moſt exquiſite contrivance was neceſſary to aſcertain the nature of it; for though, upon this occaſion, the lateral ſpark was near a quarter of an inch in length, the threads on the inſulated tube could only be made, by the exploſion, to change their poſition, from leaning a little one way, to leaning as much the other, in the neighbourhood of an inſulated braſs rod, loaded with a ſmall quantity of poſitive or negative electricity.

I could not help, however, being ſurprized, that ſo large a ſpark ſhould give no more electricity to the inſulated tube, than it appeared to have done; when, in other circumſtances, a ſpark ten times leſs than this would have made a great and permanent alteration: yet, improbable as theſe circumſtances were, I entertained no doubt at that time, but that theſe inſulated bodies were electrified, either poſitively or negatively, according as the inſide of the jar was poſitive or negative, by this lateral exploſion, though the degree was exceeding ſmall; and I continued in this perſuaſion the longer, as it happened to be a conſiderable time before I got another ſpark that communicated no ſenſible electricity. I cannot help taking notice, that if it had happened, that, in my firſt experiment, the inſulated tube had always acquired or loſt the leaſt ſenſible electricity (and, as I afterwards found, there were many chances againſt the firſt reſult) I ſhould have formed, and have acquieſced in, ſome ſort of hypotheſis, to account for the giving or receiving of electricity in thoſe circumſtances,

ſtances, and there the buſineſs would have ended; but the ſeeming contrariety of theſe appearances obliged me to purſue them farther.

Not being able completely to ſatisfy myſelf with my laſt concluſion, attended with the difficulties above mentioned, I kept diverſifying the experiments, and introducing every circumſtance that I could imagine might poſſibly affect the reſult of them; and among the reſt, I made the following experiments, which quite unhinged me again, and left me as much at a loſs as ever I had been before.

Having ſuſpended a fine thread on an inſulated braſs rod, placed about $\frac{1}{8}$ of an inch from another rod, which was likewiſe inſulated, and one end of which was in contact with the coating of the jar; and having electrified the rod that ſupported the pith balls, and placed a rod loaded with the ſame electricity near them: I obſerved that, upon every diſcharge, the balls, which before were repelled, were inſtantly attracted by the electrified rod; and, that the reſult was invariably the ſame, whether they and the rod were loaded with poſitive or negative electricity; and alſo whether the jar was charged poſitively or negatively. I repeated the experiment for ſeveral hours, without the leaſt variation in the event; which clearly proved, that, in theſe circumſtances, the electricity of the rod that received the lateral exploſion was diſcharged by it.

Afterwards, I repeated this experiment with ſome little variety, and found the electricity of the rod only leſſened by the lateral exploſion. Theſe experiments, however, by no means favoured the ſuppoſition of the uniform communication of electricity, either that of the inſide or that of the outſide of the jar; and, together with the former experiments, convinced me, that this lateral ſpark by no means produced the effect that might have been expected in communicating electricity. But, with the next ſet of experiments, the difficulty began a little to clear up, and continued to do ſo gradually, till I gained all the ſatisfaction I could wiſh for, with reſpect to this puzzling phænomenon.

The firſt time that I was able to vary the electricity of the inſulated body placed near the electric circuit, or of the bodies that formed the circuit (which I now began to attend to), by any different adjuſtment of the apparatus, was on the following occaſion.

Near to an iron rod, that touched the bottom of a jar charged poſitively, I placed another inſulated rod, with a pair of pith balls hanging to it; and obſerved, that, when I attempted to make the diſcharge, through an imperfectly conducting circuit, (bringing *e, g*, part of the table into it), a ſtrong ſpark paſſed between the inſulated rod and the other that touched the jar, and immediately the balls ſeparated as far as they poſſibly could; and, continuing in a repulſive ſtate, appeared to be electrified negatively. But immediately completing the circuit with good conductors, and making the remainder of the exploſion in a full ſpark; another ſpark paſſed between the two rods, and immediately the balls fell cloſe together again; and ſometimes would ſeparate with the oppoſite, *i. e.* poſitive, electricity.

I could not, upon this occaſion, make the lateral ſpark, in the full exploſion, ſo great as in the im-

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perfect diſcharge. I alſo obſerved, that the more interrupted the circuit was, the farther would the lateral exploſion reach; and that the electricity, which the full exploſion communicated, was always poſitive when the jar was charged poſitively, and negative when it was charged negatively. The reſult of the imperfect diſcharge was always the reverſe.

Inſulating ſeveral bodies, and the jar too, charged poſitively, they all equally contracted poſitive electricity by the diſcharge.

In this ſtate of the experiments, I had no idea of the poſſibility of the lateral ſpark not communicating electricity to the inſulated body; but I imagined that the kind of electricity communicated depended upon ſome circumſtance in the diſpoſition of the apparatus, that I was not ſufficiently aware of.

At length recollecting, that this laſt experiment reſembled, in ſome reſpects, that curious one of profeſſor Richman, mentioned in the Hiſtory of Electricity, p. 272, in which it appeared, that when the coating of either ſide of a plate of glaſs communicated with the ground, the oppoſite electricity of the other ſide was more vigorous; I was ſatisfied that the negative electricity of the bodies that formed the circuit in the imperfect diſcharge, was produced by the greater difficulty with which the outſide of the jar was ſupplied, than the inſide was diſcharged; ſo that the outſide was comparatively in a ſtate of inſulation, and therefore would communicate negative electricity to all bodies within its reach. And from this I was led to conclude, that, provided the jar was inſulated, and the inſide was made to part with its electricity with more difficulty than the outſide received it, the bodies that formed the circuit would contract poſitive electricity; and the reſult anſwered exactly my expectations.

I alſo concluded, that, making the interruption in the middle of the circuit, ſince, in this caſe, the inſide would give, and the outſide receive, with equal difficulty, the bodies in the circuit, placed between the place of interruption and the inſide of the jar, would be charged poſitively; and thoſe placed between the place of interruption and the outſide, would be charged negatively; and this alſo was verified by experiment.

In this ſtate of things, I found, that I could give the inſulated circuit what kind of electricity I pleaſed, provided there was any kind of interruption in ſome part of the circuit; and conjecturing that the electricity of bodies placed near the circuit would be the ſame with that of the bodies that compoſed it, I ſometimes placed the rod that ſupported the pith balls near the circuit, and ſometimes introduced it into the circuit; and found, that, in both caſes, it contracted the ſame electricity. This tended to confirm me in my ſuppoſition, that the lateral exploſion was always attended with a giving or receiving of electricity, according to the nature of the circuit, and the place where it was ſituated; and I again overlooked the diſproportion between the cauſe and the effect.

Preſently after this, it occurred to me, that what may be called the redundant electricity of the outſide or inſide of the jar, ſeparates from that which is in the glaſs, and conſtitutes the charge, muſt have ſome concern in this event; and the ſuppoſition was verified by fact. For, inſulating a jar, charged poſitively, I obſerved, that when I touched the outſide coating laſt (as is commonly done in ſetting it down) and made the diſcharge through good conductors, they were all electrified poſitively; and bodies placed near the circuit were the ſame. On the contrary, if, after placing the jar upon the ſtand, I touched the knob of the wire, communicating with the inſide, ſo as to take off all its redundant electricity; both the circuit and the neighbouring bodies contracted negative electricity.

I had at this time quite forgot that Æpinus had made the ſame obſervation, on diſcharging a plate of air, mentioned in the Hiſtory of Electricity, p. 273; but, conſidering what he ſays on that ſubject, I find he was miſtaken with reſpect to the reaſon of this experiment not ſucceeding with Dr. Franklin and others, who had always aſſerted, that the electric circuit contracts no electricity at all by a diſcharge. For he ſays, that the ſurfaces with which the doctor tried the experiment, were not large enough to make the effect ſenſible; and that the diſtance of the metal plates was likewiſe too ſmall, as, he ſays, it neceſſarily muſt be in the charging of glaſs: whereas I could give the inſulated circuit as ſenſible electricity with a common jar, as he could with his plate of air; and much more depends upon the height of the charge, which muſt have been inconſiderable in the plate of air, than the quantity of ſurface; which, however, may be increaſed at pleaſure, by multiplying jars in batteries.

I found, however, afterwards, that much depended upon the quantity of ſurface in the coating, and the bodies connected with them, as containing more of that redundant electricity, the effect of which was ſeen in the laſt mentioned experiment. For when I diſcharged the jar, ſtanding in contact with the prime conductor, the tendency to the communication of poſitive electricity was ſo great, that, in that ſituation, the inſulated circuit contracted ſtrong poſitive electricity, when, every thing elſe remaining the ſame, except removing it from the conductor, and then making the diſcharge, it contracted no electricity at all.

Being now perfectly maſter of the electricity of the circuit in electrical exploſions, and being able, in two different methods, to give which of the two electricities I pleaſed; I imagined that, if I could ſo balance them, as to communicate neither, there would be no lateral ſpark, as in the abovementioned experiments; but in this I was abſolutely miſtaken: For,

In the firſt place, when, after ſetting the charged jar upon the ſtand, I took off, as near as I could gueſs, one half of the redundant electricity of the inſide, and left both ſides equally electrified (as appeared by the equal attraction of the pith balls to them both), the diſcharge of the jar through a circuit of good conductors did not, indeed, communicate the leaſt ſenſible electricity to the circuit, but the lateral exploſion was almoſt as manifeſt as before. The pith balls, hung upon the rod that received it, never ſeparated.

In the next place, I repeated this experiment by balancing the two different methods of communicating electricity to the circuit, one againſt the other. For, not inſulating the jar, but ſetting it on the table, which gave the circuit and the bodies contiguous to it an advantage for contracting poſitive electricity by the diſcharge; but, at the ſame time, making an interruption in the circuit (by introducing part of the table into it, which tended to give them negative electricity); I could eaſily manage it ſo, that the circuit contracted neither the one nor the other; and yet, as in the former caſe, the lateral exploſion was as conſiderable as ever. The balls never ſeparated.

To vary the experiment, I placed an inſulated braſs ball, two inches in diameter, round and ſmooth, ſo as not eaſily to part with any electricity it had got, in the place of the rod that ſupported the pith balls; and having found a ſituation in which no electricity was communicated to the circuit, I obſerved that none was communicated to it, though, to all appearance, it received a ſpark of about $\frac{1}{4}$ of an inch in length. At leaſt, if it had contracted any, it was ſo little, as to make it very problematical; whether a pith ball, or a fine thread, was moved by it, or not: whereas, when I gave it the ſmalleſt ſenſible ſpark in any other manner, it would attract thoſe light bodies for a long time together.

The interruption of the circuit I made uſe of in this experiment, was not by means of any part of the table, but only about a yard of braſs chain introduced into it, and diſpoſed between the inſide of the jar and that part of the circuit, near which the

inſulated

inſulated ball was placed. N.B. The ball muſt not be placed near the jar itſelf; for, in that ſituation, I found, that, though it was very ſmooth, and perfectly ſpherical, yet it could not be placed very near the outſide of the jar ſtanding on the table, without contracting negative electricity, in a very ſmall ſpace of time.

Theſe experiments threw me back into my former ſtate of perplexity, with reſpect to the lateral ſpark; ſince, when the two electricities of the circuit were exactly balanced, it was very little diminiſhed, and yet the body that received it was not in the leaſt ſenſibly electrified. But, upon reflection, I concluded, that this lateral ſpark muſt be of the nature of an exploſion, and conſequently, that an electric ſpark muſt enter, and paſs out again, within ſo ſhort a ſpace of time, as not to be diſtinguiſhed, and leave no ſenſible effect whatever: for though, in this caſe, part of the electric matter natural to the body muſt be repelled, to make room for the foreign electricity, its reſtoration to its natural ſtate was ſo quick, that no other motion could correſpond to it.

This hypotheſis is favoured by the obſervation, that it is the very ſame thing whether a body be introduced into a circuit, or placed near it, with reſpect to contracting electricity; that is, whether the electric charge enter the body at one place, and go out at another, or whether it be received and emitted at the ſame place.

This lateral exploſion is an effect ſimilar to a partial circuit, in which, part of the electric matter that forms the charge in an exploſion, goes one way, while the reſt of the charge goes another; the only difference is, that this detached part of the charge leaves the common track, and returns to it again, in the very ſame place.

Several remarkable partial circuits occurred in the courſe of my experiments before, particularly one, mentioned in the Hiſtory of Electricity, p. 692, in which, part only of the exploſion paſſed in the ſhorteſt way, while another part of it took a circuit, conſiſting of the ſame materials, thirty times as long; and another, mentioned, p. 691, where one circuit was made through a thick rod of metal, and another, at the ſame time, through the open air.

That there is an admiſſion and an exp~~lo~~ulſion of the electric matter, in this lateral exploſion, ſeems evident, from this circumſtance, that it is far more conſiderable when the body that receives it is large, than when it is ſmall. In the former caſe, there is room for the electric matter, natural to the body, to retire, upon the admiſſion of the foreign electricity belonging to the charge; whereas, in the latter caſe, there is not room for it. When I placed a ſmall braſs ball, of about a quarter of an inch in diameter, near the circuit, I could not perceive that it was at all affected by any lateral exploſion; and the ſpark was very inconſiderable, when I placed a needle, about two inches in length, to receive it; but when I connected the large tube above mentioned, by means of a pretty thick iron wire, to any body whatever, that was placed in the neighbourhood of the circuit, I have (with a jar of only half a ſquare foot of coating glaſs) made the lateral exploſion, an inch or more in length, conſiſting of a very full and bright ſpark of electric fire. Inſulated bodies, of about eight or nine feet in length, ſeem to admit as large a lateral exploſion as any body whatever is capable of: for, connecting them with the ground, by means of the beſt cōductors (which gave the electric matter in the bodies, the freeſt receſs poſſible) I could never make this exploſion much more conſiderable, uſing the ſame jar, and all other circumſtances the ſame.

It is a manifeſt advantage in theſe experiments, that the lateral exploſion be not taken from the coating of the jar itſelf, or from any part of the circuit, very near to it. I have found that, *cæteris paribus*, it is the moſt conſiderable taken at the extremity of a braſs rod, of one foot, or a foot and a half long, the other end of which is contiguous to the jar. It is analogous to this, that the longeſt ſpark is taken, not from the body of the prime conductor itſelf, but at the extremity of a long rod inſerted into it. The electric matter ſeems to acquire a kind of *impetus* by the length of the medium, through which it paſſes. But I found that the *maximum*, in this caſe, did not exceed, or rather, that it did not quite reach, three feet; for, making uſe of a thick iron rod, eight or nine feet long, the lateral exploſion, taken at the extremity of it, was about the ſame, as when it was taken at the end of a rod four inches from the jar; and not half ſo conſiderable as when taken at the extremity of a rod one foot long. This, I imagined, might be owing to the obſtruction which the electric fluid meets with in paſſing even through metals; which appears, by my former experiments, to be much more conſiderable than was generally imagined.

Upon

Upon the whole, this remarkable experiment ſeems to be made to the moſt advantage in the following circumſtances. Let the jar ſtand upon the table; let a thick braſs rod, inſulated, ſtand contiguous to the coating; and, near the extremity of this rod, place the body that is to receive the exploſion. This body muſt be ſix or ſeven feet in length, and, perhaps, ſome inches in thickneſs, or be connected with a body of thoſe dimenſions. Laſtly, let the exploſion be made with the diſcharging rod reſting upon the table, cloſe to a chain, the extremity of which reaches within about an inch and a half of the coating of the jar. In this caſe, the operator will hardly fail of getting a lateral exploſion of an inch in length; which ſhall enter and leave the inſulated body, without making any ſenſible alteration in the electricity natural to it.

With large jars, containing three or four ſquare feet of coated glaſs, bearing a very high charge, I make no doubt but that this experiment might be made to much more advantage; but, at the time that I was engaged in this inveſtigation, I happened not to have any ſuch jar, and therefore only uſed one that contained half a ſquare foot of coated glaſs.

If the interruption in the circuit, which is almoſt neceſſary in theſe experiments, be made by introducing a length of chain into it, rather than by making part of the exploſion paſs along the tube, there is a medium in the length of chain, that anſwers better than either a longer or a ſhorter circuit. In a long interrupted circuit, the electric matter ſeems to loſe the *impetus* which it diſcovers in a ſhort one.

In all theſe caſes, the electric charge ſeems to remain for a moment in the parts of the interrupted circuit; and therefore inſtantaneouſly ruſhes, in all directions, as well towards bodies that are not placed along its paſſage to the jar, as thoſe that are; but, when the ſame charge occupies a larger circuit, it has more room to expand itſelf, and is not ſo ſtrongly impelled to deſert it. I found, however, by repeated trials, that when I made uſe of three yards of braſs chain in the circuit, there was a diſtance to which the lateral exploſion would not reach. The ſame diſtance it alſo would not reach, when the circuit conſiſted of only one braſs rod; but it reached it with great eaſe, when only half a yard of chain was uſed, even without any other interruption in the circuit. But it reached to a much greater diſtance, when the chain was very ſhort, and the interruption was greater in other reſpects.

I had imagined, that, ſince the body which had received the lateral exploſion contained, for a moment, more than its natural quantity; that, if it were acutely pointed, ſome would eſcape, and that, upon the return of the exploſion, the body would be exhauſted; but I found no ſuch effect, though I affixed fine needles to the bodies I made uſe of. The lighteſt pith balls, placed near the extremities of theſe needles, were not in the leaſt affected by the exploſion.

When I placed a number of braſs balls, one behind another, the lateral exploſion paſſed through them all, being viſible in the intervals between each of them, and returned the ſame way, leaving them all in the ſame ſtate in which it found them; and a great number of lateral exploſions might be taken at the ſame time, in different parts of the circuit, ſome of them very near one another.

It made no difference, whether the lateral exploſion was received on a flat ſmooth ſurface, or the points of fine needles. In both caſes, the ſpark was equally long and vivid.

I had no ſooner completed theſe experiments on the lateral exploſion; but I had a curioſity to ſee what kind of an appearance it would make *in vacuo*; ſince no other phænomenon in electricity reſembles it. In all other caſes, the electric matter ruſhes in one ſingle direction; whereas, in this, it goes and returns in the ſame path, and, as far as can be diſtinguiſhed, at the ſame inſtant of time; ſo that all the difference of the two electricities, which are ſo conſpicuous *in vacuo*, muſt here be confounded. Accordingly, I found, though my pump was not in good order, that I could perceive this exploſion *in vacuo*, at the ends of rods, placed ſeveral inches aſunder; and when they were brought within about two inches, they ſeemed to be joined by a thin blue or purple light, quite uniform in its appearance. As theſe rods were made to approach, this light grew denſer; but ſtill exhibited no ſuch variety, as is obſerved between the bodies that give and receive electricity, in the common experiments *in vacuo*.

I was pretty ſoon convinced, that uncoated jars could not be uſed to any more advantage in theſe experiments, than thoſe that were coated; ſince the want of coating only operated as an interruption in the circuit, occaſioning a difficulty in the admiſſion of the charge on the outſide of the jar. And, in all caſes, the greater this difficulty of paſſage was made, provided the diſcharge was made at once, the more conſiderable was the lateral exploſion, and the greater ſhock was given to the hand that held the diſcharging rod; which ſhock was nothing more than one of theſe lateral exploſions, iſſuing from the rod as part of the circuit.

I ſhall conclude the account of theſe experiments with obſerving, that they may poſſibly be of ſome uſe in meaſuring the conducting power of different ſubſtances; ſince, the greater is the interruption in the electric circuit, occaſioned by the badneſs of its conducting power, the more conſiderable, *cæteris paribus*, is the lateral exploſion.

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XIX. *Experiments and Obſervations on Charcoal: By* Joſeph Prieſtley, *LL. D. F. R. S.*

Read April 5, 1770.

AMONG the original experiments, publiſhed in the Hiſtory of Electricity, was an account of the conducting power of charcoal. This ſubſtance had been conſidered by electricians, in no other light than that of more perfectly baked wood, which is known to be no conductor of electricity. I have even heard of attempts being made to excite it; and though thoſe attempts were ineffectual, the failure of ſucceſs was attributed to other cauſes than that of charcoal being no electric ſubſtance; ſo fixed was the perſuaſion, that water and metals were the only conducting ſubſtances in nature. The conſideration of the chemical properties of charcoal, which are, in many reſpects, remarkably different from thoſe of the wood from which it is made, might have led them to ſuſpect, that ſince, after its being reduced to a coal, it was become quite another thing from what it was before, it might poſſibly differ from wood in this property; but this conſideration had not been ſufficiently attended to.

In the account of my former experiments on charcoal, I obſerved, that there were very great differences in the conducting power of charcoal, and particularly of wood charcoal, though I could not determine on what circumſtances in the preparation, &c. thoſe differences depended. I thererefore expreſſed a wiſh, that ſome perſon, who had conveniencies for making chemical experiments, would proſecute the inquiry, as one that promiſed, not only to aſcertain the cauſe of the conducting power of charcoal, but perhaps of conducting power univerſally. Not hearing that any chemiſt or electrician has attended to this buſineſs, I have, at length, reſumed the ſubject, though not with every advantage that I could have wiſhed. I have, in a great meaſure, however, ſucceeded in the principal object of my inquiry; and I ſhall now lay before this Society, the reſult of my experiments and obſervations.

I ſhall begin with correcting a miſtake I lay under at the time that I made the former experiments. Having been informed by perſons, who attend the making of pit charcoal, that it was conſiderably increaſed in bulk after the proceſs; I imagined that all other ſubſtances received an increaſe of bulk, when they were reduced to a coal; but the firſt experiments that I made, convinced me of my miſtake. All vegetable ſubſtances are conſiderably contracted in all their dimenſions, by the proceſs of coaling, and the more perfect this proceſs is, that is, as will be explained hereafter, the greater heat is applied in the courſe of it, the greater is the diminution. I have even reduced pieces of wood to little more than one fourth of their original length and breadth, in a common fire, by the uſe of a pair of hand bellows only. And this was the caſe equally with wood of the firmeſt texture, as ebony; that of a middle texture, as oak; and that of the looſeſt, as fir, &c.

As moiſture, and, I believe, ſmall degrees of heat or cold, affects wood much more ſenſibly acroſs the fibres than along them, it might have been ſuppoſed, that when wood was reduced to a coal, by the application of a greater degree of heat, the ſame rule would have been obſerved; but I found very little difference in this reſpect. To aſcertain this circumſtance, I took, from the ſame board, two pieces, each 2 ½ inches in length. In one of them, the fibres were divided, in the other they were not; and after coaling them thoroughly together, in the ſame crucible, I found that the former meaſured 2.05 inches, and the latter 2.15. Their conducting power could not be diſtinguiſhed.

A more particular account of the degree, in which wood is ſhortened in coaling, will be ſeen afterwards, when the variations in this reſpect are compared with the variations in the power of conducting electricity.

To my great ſurprize, I found animal ſubſtances not reduced in their dimenſions by the proceſs of coaling. This, at leaſt, was the caſe with ſome pieces of ivory, ſeveral inches in length, and a piece of bone. They bore a very intenſe heat for many hours, and came out of the crucible conſiderably diminiſhed in weight, but hardly ſo much as diſtorted in their ſhape, as is remarkably the caſe with wood, and, I believe, all vegetable ſubſtances.

In examining mineral ſubſtances, I found that my information, mentioned above, was juſt. Coals are very much inlarged in their dimenſions by charring; but the experiment muſt be made with great care, to judge of this circumſtance; for, unleſs the operation be very ſlow, the coal will retain nothing of its former ſhape, having been made in ſome meaſure fluid by the heat. The inſide of all pieces of pit charcoal is full of cavitities, and there is generally a very large one in the center of every piece; ſo that, the dilatation is nothing like the extenſion of fibres; but is produced by the elaſticity of the new formed vapour, in forcing its way out, while the ſubſtance is ſoft.

With reſpect to the main object of my inquiry, I preſently ſatisfied myſelf, that the conducting power of charcoal depends upon no other circumſtance than the degree of heat, that is applied in the proceſs of making it. I had not ſuſpected this; but numberleſs experiments clearly proved it. Taking an iron pot filled with ſand, and putting into it pieces of wood, cut out of the ſame plank, marking them, and carefully noting their places in the pot, I always found that thoſe pieces came out the beſt conductors, that had been expoſed to the greateſt heat. The reſult was the ſame when I made coals of bits of wood, placed one above another, in a gun barrel, one end of which was made red hot, and the reſt gradually cooler and cooler.

Taking pieces of charcoal that conducted very imperfectly, or not at all, I never failed to give them the ſtrongeſt conducting power, by repeating the proceſs of coaling, either in a crucible, or a gun barrel, covered with ſand, and kept in an intenſe heat.

I could not find that the mere continuance of the ſame degree of heat had any effect with reſpect to the conducting power of charcoal.

Macquer and other chemiſts define charcoal to be wood burned, without being ſuffered to flame; but, with reſpect to its conducting power, and, I make no doubt, with reſpect to all other eſſential properties alſo, it makes no difference whether it flame or not. I have coaled pieces of wood, both in gun barrels, and in crucibles, ſlightly covered with ſand, and have let the inflammable vapour that exhaled from them take fire, at various diſtances from the ſubſtances; and I have alſo put pieces of wood in an open fire, and urged the heat applied to them, with a pair of bellows; and in all theſe caſes have found the charcoal equally good. In the laſt method, indeed, very little of the ſubſtance is preſerved; but the little that doth remain, after it has ceaſed to flame, whether it be quenched immediately, or not, conducts as well as any charcoal whatever. But one can hardly be ſure that the ſame degree of heat is given to every part of a piece of wood, except it be expoſed to it for ſome time; and in an open fire, urged with a pair of bellows, the wood waſtes as faſt as it is red hot, before the center of it is much affected with the heat.

When once any degree of conducting power is given to a piece of charcoal, I never found that it was afterwards leſſened. A partial conſuming of it in an open fire doth not affect the remainder, as I obſerved in the account of my former experiments.

I had imagined, that the ſolidity of ſubſtances converted into charcoal would have had a very conſiderable effect on their conducting power afterwards; but the conjecture was not confirmed by experiment. Coals made of the lighteſt woods conducted, as far as I could perceive, as well as thoſe that were made from the moſt ſolid, if they had been expoſed to the ſame degree of heat in the proceſs. Fine ſhavings of fir, the thin coats of an onion, the lighteſt ſort, and every other vegetable ſubſtance that I tried, conducted equally with coals made of oak or ebony.

I had imagined, alſo, that the moment a piece of wood was become black with heat, it was, to all intents and purpoſes, a real charcoal, and, along with other properties of charcoal, would conduct electricity, more or leſs: but I found, by coaling ſeveral pieces very ſlowly, that they would not conduct in the leaſt degree, not only when they were made ſuperficially black, but likewiſe when they were black quite through, and had remained a long time in the heat that made them ſo; ſo that no eye could diſtinguiſh them from the moſt perfect charcoal.

I have ſometimes found charcoal in ſuch a ſtate, that it would aſſiſt the paſſage of an exploſion along its ſurface, when it would not conduct a ſhock any other way.

In order to ſatisfy myſelf in what proportions the diminution of weight, the decreaſe of bulk, and the conducting power of wood and charcoal, correſponded to one another. I took ſeveral pieces from the ſame plank, and having carefully weighed and meaſured them, converted them into coals very ſlowly, and by a gradual increaſe of heat, on an iron plate, held on the fire, turning them conſtantly, to prevent their catching fire. The following were the reſults.

A piece of very old dry oak, weighing 12 grains, and which conducted in the imperfect manner that wood generally does, from the moiſture it contains, was, after the loſs of about one grain, no conductor at all; and it continued the ſame as baked wood, till it was reduced to 4 grains, when it was black quite through; and even then, no part of it conducted, except one corner, where it had catched fire.

Another piece I carefully weighed and meaſured ſeveral times, in the courſe of the proceſs. At firſt it weighed 12 grains, when its dimenſions were 2 inches and .45. At 8 grains they were 2 and .4; at 5 .5 gr. 1 .91 and .4; at 3 .5 gr. 1 .8 and .35. It was now become an imperfect conductor. I then urged it with a ſtrong heat, in a crucible, and taking it out, it weighed 1 .75 gr. and meaſured 1 .6, and .3. It was now a perfect conductor; and though I afterwards kept it in a very intenſe heat ſeveral hours, by which it was reduced to 1 gr. and meaſured 1 .6, and .3, its conducting power was not ſenſibly increaſed; but it was become very brittle or friable.

It appears from theſe experiments, that theſe pieces of wood were reduced to about $\frac{1}{4}$ of their weight before they would conduct at all; though, at the ſame time, they were diminiſhed in length (*i. e.* along the fibres) only $\frac{1}{10}$. The breadth and thickneſs could

not

not be meaſured with ſufficient accuracy in theſe ſmall pieces. To make them perfect conductors, they were reduced to about $\frac{1}{10}$ in weight, and $\frac{1}{2}$ in length.

A variety of circumſtances led me to conclude that the cauſe of blackneſs, and of the conducting power in charcoal, is the oil of the plant, made empyreumatic, and burnt to a certain degree. I therefore conclude that theſe properties are ſome way connected with that part of the inflammable principle, otherwiſe called phlogiſton, that is fixed and united to the earth of the plant, when the union is ſtrengthened by an intenſe heat.

The ſand, with which I covered the ſubſtances that I converted into coals, and alſo the pipe clay which I ſometimes put over them, contracted a blackneſs like charcoal, and would often conduct pretty well. Sometimes they would conduct a ſhock. This muſt have been owing to the oil they received from the ſubſtances out of which it was expelled by the heat. In the experiment of the gun barrel filled with pieces of wood, mentioned above, the uppermoſt pieces were not in the leaſt burnt. They could hardly have been hot; yet, having contracted a ſuperficial blackneſs, from the vapour of the oil expelled from the piece below them, they would even conduct a ſhock, though not in the moſt perfect manner.

Sometimes thoſe ſubſtances that had no phlogiſton themſelves, but received it in conſequence of being placed in the neighbourhood of other bodies out of which it was expelled, would not conduct immediately; but would be made to do ſo by being expoſed to a greater heat, which more thoroughly burnt the oil with which their pores were filled.

I put a piece of common pipe into a crucible, in which I was burning ſome turpentine (which will be mentioned below); and it came out black quite through, like a pipe in which tobacco has been frequently ſmoaked. In this ſtate it would not conduct at all; but, putting it into a crucible, covered with ſand, I treated it in the ſame manner as I would have done a piece of wood, in order to coal it, and it came out a very good conductor. Had it been burned in the open fire, the phlogiſton would have eſcaped, and the pipe would have been left white as at firſt.

Being convinced that the conducting power of charcoal depended upon the oil, or rather the phlogiſton contained in the oil, and on the degree of heat with which it was burned, I took ſeveral methods to give vegetable ſubſtances more of this principle; or at leaſt endeavoured to make them retain more of it than they uſually do, in the proceſs of coaling. But I had no apparent ſucceſs in thoſe experiments.

I began with plunging a piece of old dry oak in oil; and then, pumping the air out of it, let it ſtand *in vacuo* a day and night, in which time it ſeemed to diſcharge a great quantity of air; after which, I let into the receiver the air, and thereby forced the oil into its pores. But the coal from this wood was not ſenſibly better than others. The application of heat may perhaps expell the phlogiſton in ſuch a manner, that the reſiduum, being fully ſaturated, can retain no more than a certain proportion. I made coals of other pieces of wood, when they were covered with cement; and I alſo coaled ſeveral pieces together, that they might receive phlogiſton from one another; but, in both caſes, without any ſenſible improvement in the quality of the coal.

In order to prevent the eſcape of the phlogiſton belonging to the ſubſtance to be reduced to a coal, I put ſome pieces of wood into a gun barrel, and corked it as cloſe as I could, at the ſame time covering the cork with cement. In this caſe the rarefaction of the exhaling vapour never failed to drive the cork out; but it muſt have been after a conſiderable reſiſtance to its eſcape. However, I could not perceive any peculiar excellence in the charcoal made in this manner.

I do not, indeed, know any method in which differences in ſubſtances that conduct ſo well as theſe can be accurately tried, at leaſt none that can be applied in this caſe. The charcoal I can make in a common fire, by the uſe of a pair of hand bellows, I cannot diſtinguiſh, with reſpect to its conducting power, from the moſt perfect metals, gold and ſilver; either by the length of the electric ſpark, the colour of it, or the ſound of the exploſion. I make no doubt but that wood, in the proceſs of coaling, may eaſily have a degree of conducting power communicated to it, exceeding that of lead, iron, or the other more imperfect metals.

We may, perhaps, be guided in our conjectures on this ſubject, by conſidering the degree of heat that is neceſſary, either to unite the phlogiſton to its baſe, or to ſeparate them, both in the caſe of wood, and the different metals. Lead is very eaſily calcined, and it is alſo known to conduct electricity very imperfectly. Iron ſoon turns to ruſt; and its conducting power I found to be very ſmall, in compariſon with that of copper, or the more perfect metals. If, therefore, in making charcoal, a degree of heat be applied greater than is neceſſary to calcine or revivify a metal, we may perhaps conclude, that the conducting power of the charcoal will be ſuperior to that of the metal. As it may be poſſible to give charcoal, when cut off from any communication with the external air, a greater degree of heat than ſilver or gold would bear, without being diſſipated in vapour; it may even be poſſible to make charcoal that ſhall conduct electricity better than thoſe moſt perfect metals.

Had there been any phlogiſton in water, I ſhould have concluded, that there had been no conducting power in nature, but in conſequence of ſome union of this principle with ſome baſe. In this, metals and charcoal exactly agree. While they have the phlogiſton, they conduct; when deprived of it, they will not conduct.

I believe, however, that all vegetable, or animal ſubſtances, that contain phlogiſton may be reduced to a coal; and if the heat applied in the proceſs be ſufficient, that coal will conduct electricity. Fleſh, glue, bones, and other parts of an animal body, make good conducting charcoal.

The only approach, or ſeeming approach, I ever made towards retaining more phlogiſton than uſual, in wood reduced to a coal, was by the ſlowneſs of the proceſs. For I always found that, if the heat was applied very gradually, leſs volatile phlogiſton, *i. e.* leſs *i. e.* leſs inflammable air, was expelled; and therefore I ſuppoſe that more of it was fixed. I could never afterwards, by equal degrees of heat, make this coal to weigh as little as another that was firſt coaled by a ſudden heat.

I took two pieces of dry oak, the contiguous parts of the ſame ſtick, each weighing exactly 14 grains. One of theſe I heated ſuddenly. It yielded 8 ounce meaſures of inflammable air, and then weighed 2 grains. The other I heated ſlowly, but as vehemently, at the laſt, as the other. It yielded only 1½ ounce meaſures, and weighed 3 grains.

I repeated the ſame experiment ſeveral times, and always with nearly the ſame reſult.

Examining the conducting power of the pieces of charcoal, made with theſe different circumſtances in the proceſs, I could not diſtinguiſh which were better. Perhaps a more accurate method of trying them might ſhow, that thoſe which were coaled ſlowly were the better conductors; unleſs, which is not improbable, the goodneſs of the conducting power conſiſt in the completeneſs of the union that is produced between the inflammable principle and its baſe, which will depend upon the degree of heat only, and not on the quantity of phlogiſton thus united to the earth.

N. B. To catch the inflammable air, ſet looſe in making charcoal, I put the ſubſtances into a gun barrel, to which I luted a long glaſs tube, and to the tube I faſtened a bladder, out of which the air was carefully preſſed.

As metals and charcoal agree in conſiſting of phlogiſton united to an earthly baſe, and alſo in conducting electricity, I ſuſpected that theſe two different ſubſtances might alſo agree in their readineſs to expand by heat. Mr. Smeaton was ſo obliging as to aſſiſt me in my attempts to aſcertain this circumſtance, by the application of his excellent pyrometer. Though we could not make the experiment with all the exactneſs that we could have wiſhed, yet the reſult of near thirty trials was uniformly in favour of the greater degree of expanſion, by heat in the charcoal, than in wood of the ſame kind, as we imagined, out of which it was made. In general, the expanſion of the charcoal was about double to that of the wood.

It is evident that a certain degree of heat makes wood and charcoal expand, and alſo that a greater degree of heat makes them contract. I wiſh we had an inſtrument to aſcertain the preciſe degree of heat, at which the expanſion ceaſes, and the contraction begins; and whether the two effects be produced by the ſame gradation.

In the courſe of theſe experiments on charcoal, I met with a ſubſtance, the conducting power of which is ſingular, and exhibits a beautiful appearance. In order to ſee what would remain after burning a quantity of turpentine in a glaſs tube, I covered it with ſand, in a crucible, in the ſame manner in which I uſed to make charcoal; and, after letting it continue a ſufficient time, in a very hot fire, and the flame had long ceaſed, I examined the tube, and found that it had been melted; but, inſtead of any thing like charcoal, or the leaſt blackneſs, I obſerved that the tube was uniformly lined with a whitiſh gloſſy matter, that I could not ſcrape off. Upon trying whether it would conduct electricity, I found it tranſmitted the ſmalleſt ſhocks, to a conſiderable diſtance; and, what appeared very remarkable, the path of the exploſion was luminous all the way, and ſeemed to conſiſt of a prodigious number of ſmall ſeparate ſparks, ſcattered to a great diſtance, exhibiting ſuch an appearance as would be made by firing gunpowder ſcattered careleſsly in a line. The exploſion very much reſembled the firing of a ſquib. To compare it to another electric appearance, it was like the exploſion paſſing through a thin ſurface of gilding.

I imagine that, though I could not perceive any interruption in this white coating, not even by the help of a microſcope, it muſt, in fact, have been full of interſtices, and the electric ſparks could only be viſible in paſſing from ~~and~~ one conducting ~~one~~ particle to another.

In this experiment, I often got pieces of glaſs very imperfectly covered, with intervals in the white coating very large and viſible; but, though I expoſed the ſame pieces of glaſs to catch more of this matter, I never could get a coating of it ſo thick, but that, in tranſmitting the electrical exploſion through it, it exhibited the ſame luminous appearance, as if there were interſtices in the circuit.

I got the ſame matter from oil of turpentine, and oil of olives; but not from bees-wax, or ſpermaceti oil; perhaps not from any animal ſubſtance.

In order to obſerve the progreſs of this incruſtation, I poured oil of turpentine on ſome flat pieces of glaſs, and burnt them on an iron plate, in the open fire, the heat being moderate; but the effect was a black covering, like ſoot, which would not conduct in the leaſt. But theſe ſame pieces of glaſs, thus covered with the black coating, being put into a crucible full of ſand, and urged with a ſtrong heat, came out white, and conducted exactly as before.

With a leſs degree of heat the black covering was changed to white, but it did not adhere ſo firmly to the glaſs, as when the heat had been greater; though it adhered more cloſely than the black covering, which might be wiped off with a feather. But this white coating, produced by a moderate heat, would not conduct at all.

In ſome caſes I have found this whitiſh matter to be diſperſed by ſeveral exploſions, as Mr. Franklin found gilding with leaf gold to be.

In whatever manner the pieces of glaſs were covered, the coating vaniſhed when it was made red hot in an open fire; and the glaſs that remained would not conduct, any more than it did before. This circumſtance exactly reſembled the eſcape of phlogiſton from charcoal and metal, burnt in the open air.

In a microſcope, this whitiſh matter looked exactly like metal, or rather ſome of the ſemi-metals, having a bright poliſh, tho' it ſoon became, as it were, tarniſhed.

To try whether it was metal, I dipped the pieces of glaſs that were covered with it in the acids, but found that they had little or no effect upon it, though it is by no means fixed in the pores of the glaſs, but covers it quite ſuperficially.

It was not in the leaſt affected by the magnet. Upon the whole, the matter that forms this coating of the glaſs ſeems to be a kind of charcoal, only white inſtead of black.

Conſidering that metals reſemble charcoal, in that they conſiſt of an earth united to phlogiſton, and that charcoal will not conſume without burning in the open air (there being, probably, ſomething in the atmoſphere with which it can unite, on the principle of chemical affinities, the moment it is ſeparated from the metallic baſe) I imagined that metals might not calcine or vitrify except in the ſame circumſtances, and the event verified my conjecture.

I took a certain quantity of lead, and having put it into an open crucible, obſerved that it was all vitrified in 10 minutes; but the ſame quantity of lead, covered with pipe clay, and ſand, was kept ſeveral hours in a much hotter fire, and was hardly waſted at all, the bottom of the crucible only being ſlightly glazed; it having been impoſſible wholly to exclude all acceſs of air, and ſome being neceſſarily in contact with it when the proceſs began. Treating charcoal in the ſame manner, I could never prevent ſome loſs of weight, when the crucible was kept in a very hot fire, for ſeveral hours.

As, by this proceſs, lead will bear a much greater degree of heat than would calcine or vitrify it in the open air, I ſhould think it probable, that lead thus prepared muſt have the phlogiſton more cloſely united to its earthy baſe, and be thereby a better conductor than common lead; ſince this is the caſe with charcoal thus treated. Perhaps lead, and other baſe metals, may have their quality altered, and be improved in other reſpects alſo by this pro~~of~~ eſs; though they ſhould not be changed into gold by it. The ſpecific gravity is not changed by this proceſs; ſo that, alas! it is ſtill lead.

XX. *Meteorological Obſervations for* 1769, *made at* Bridgewater *in* Somerſetſhire. *Communicated by Dr.* Jeremiah Milles, *Dean of* Exeter, *and F. R. S.*

Read April 5, 1770. Received February 8, 1770.

METEOROLOGICAL OBERVATIONS at BRIDGEWATER, 1769.

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.		Ombr.
				Med.	Inches.
January	8 Higheſt 30,22 24 Loweſt 29.35	The firſt 9 days chiefly cloudy, the next 7 days ſun ſhine and ſtorms intermixed, then froſt for 3 days, the remainder of the month chiefly ſtormy. The firſt part of the month very calm; the latter, high winds; the firſt S.W. and N.E the latter S. E. and N. W.	Higheſt 30 53 Loweſt 22 26	$38\frac{9}{31}$	$1\frac{980}{1000}$
February	20 Higheſt 30,21 4 Loweſt 29,11	The firſt part cloudy and ſun ſhine intermixed, the latter part cloudy and ſtormy. The wind chiefly N. W. the whole month; the firſt part and the latter part high, the middle moderate.	Higheſt 8 52 Loweſt 1 31	$40\frac{11}{28}$	2,540
March	25 Higheſt 30,50 11 Loweſt 28,99	To the 14th, cloudy and ſtormy, from thence to the end cloudy and ſun ſhine, intermixed. The firſt part, the middle, and the latter end, high winds, the intermediate parts moderate. The firſt part Weſterly winds, then S. E. he latter part nearly equally divided between N. E. and N. W.	Higheſt 4 53 Loweſt 31 29	$43\frac{17\frac{1}{2}}{31}$	1,155

April

Month.		Barometer.		State of the Weather and Wind.	Fahrenheit's Thermom.				Ombr.
								Med.	Inches.
April	30 11	Higheſt Loweſt	30,33 28,68	The firſt 18 days ſun ſhine and cloudy intermixed, the other 12 days continued ſun ſhine. The firſt and latter parts very moderate winds, the middle high, and remarkably high in the night of the 13th. The firſt 8 days N. E. the middle Weſterly, and the latter part N. W.	Higheſt Loweſt	30 1	65 32	$49\frac{5}{30}$	,978
May	2 27	Higheſt Loweſt	30,54 29,34	The firſt 16 days chiefly ſun ſhine, from thence to the 26th cloudy and ſun ſhine intermixed, the latter part ſtormy. The firſt 11 days the wind N. W. the next 10 N. E. and the latter part divided between N. W. and S. W. The wind moderate till the 25th, the remainder high.	Higheſt Loweſt	22 12	70 43	$52\frac{1}{2}$	2,758
June	12 6	Higheſt Loweſt	30,23 29,42	The whole month in general cloudy and ſtormy, with a few days of ſun ſhine about the middle. The wind for the moſt part high, and nearly equally divided between S. W. and N. W.	Higheſt Loweſt	27 12	67 51	$57\frac{1}{2}$	$3,252\frac{1}{2}$
July	4 17	Higheſt Loweſt	30,23 29,68	To the 24th chiefly ſun ſhine; from thence cloudy and ſtormy, moderate winds in general, higheſt towards the latter end, generally N. W.	Higheſt Loweſt	7 1	74 52	$64\frac{6}{31}$	$1,247\frac{1}{2}$
					Auguſt				1,948
					September				1,215
					October				,680
					November				$3\cdot122\frac{1}{2}$
					December				$2\ 782\frac{1}{2}$

Total Rain this Year 1769. $23,\frac{659}{1000}$

Received February 8, 1770,

XXI. *Meteorological Obſervations at* Ludgvan *in* Mount's-Bay, Cornwall, 1769: *By* William Borlaſe, *D. D. F. R. S. Communicated by Dr.* Jeremiah Milles, *Dean of* Exeter, *and F. R. S.*

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.		Ombr.
				Med.	Inches.
January	Higheſt 7 30,12 Loweſt 24 29,10	Calm, cloudy, and ſhowery to the 14th, the reſt ſtormy and windy, rain; ſleet on the 30th; calm and a ſlight froſt on the 31ſt. Wind from all points nearly equal.	Higheſt 28 $51\frac{1}{2}$ Loweſt 21 33	$43\frac{22}{31}$	$4\frac{690}{1000}$
February	Higheſt 20 30,5 Loweſt 4 29,3	Rainy and ſhowery, without froſt, ſnow, or hail, to the 8th; on the 9th and 10th hail-ſhowers with ſome ſleet; hail again on the 22d and 24th; the reſt rainy, windy, ſtormy; wind 17 days from the North, the reſt moſtly from the Weſt.	Higheſt 27 57 Loweſt 12 35	$44\frac{1}{28}$	4,640
March	Higheſt 24 30,40 Loweſt 11 28,83	Showery, not much rain; windy, rather than ſtormy; fair 12 days; wind 17 days from the Weſt, reſt nearly equal.	Higheſt 19 52 Loweſt 31 34	$46\frac{7}{31}$	2,240
April	Higheſt 30 30,21 Loweſt 9 28,66	Showers of ſnow and hail on the 1ſt; ditto on the 4th with ſunſhine; none on any other day, nor then, any lying; calm and fair 12 days, the reſt windy; about the middle ſtormy. Wind 17 days North, reſt nearly equal.	Higheſt 29 66 Loweſt 1 37	$49\frac{8}{30}$	1,690

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.		Ombr.
May	Higheſt 2 30,36 Loweſt 27 29,23	Calm and fair to the 11th, ditto 15, 16, 21, 23, 24, 25, 29, 30; reſt windy with rain. Wind 19 days from the North, 15 from the Eaſt, South and Weſt equai.	Higheſt 4 67 Loweſt 12 46	$53\frac{13}{31}\frac{1}{2}$	Inches. 4,010
June	Higheſt 19 30,10 Loweſt 16 29,47	Rainy, windy and ſhowery; 8 days only calm mixed with fair, and ſunſhine. Wind 15 days North, 19 days Weſt, reſt nearly equal.	Higheſt 30 $64\frac{1}{2}$ Loweſt 3 49	$55\frac{8}{30}\frac{1}{2}$	4,100
July	Higheſt 25 30,13 Loweſt 16 29,58	Half this month we had calms, partly with fair weather, and partly with fogs and miſts, reſt variable, without extremes; 24 days the wind blew from the Weſt, mixed with other winds nearly equally.	Higheſt 5 72 Loweſt 19 55	$60\frac{30}{31}$	1,320
Auguſt	Higheſt 17 30,3 Loweſt 30 29,28	Twelve days we had calms; ſome fair, ſome cloudy, ſome ſhowery, reſt windy, rain and ſhowers. Wind blew 26 days from the Weſt, 18 from the South, reſt more North than Eaſt.	Higheſt 3 67 Loweſt 23 51	$58\frac{3}{4}$	3,550
September	Higheſt 30 30,22 Loweſt 12 29,20	Seventeen calm days, reſt a mixture of all. Wind 26 days from the Weſt; as to the reſt, the North prevailing.	Higheſt 6 65 Loweſt 30 50	$56\frac{6}{30}\frac{1}{2}$	3,700
October	Higheſt 13 30.20 Loweſt 31 29,1	Half this month fair and calm; reſt fogs, windy and rains. Wind 26 days from the Eaſt, 16 days from the North, South and Weſt nearly equal.	Higheſt 1 59 Loweſt 6 42	$50\frac{7}{31}$	2,250
November	Higheſt 27 30,40 Loweſt 8 29,12	Stormy, rainy, few miſts or calms, froſt only one morning, the 19th; a ſtorm on the 4th; a violent ſtorm on Friday the 24th, the extreme between ten and twelve P. M. Wind moſtly Weſt (viz. 18 out of 30), reſt mixed.	Higheſt 3 57 Loweſt 19 36	$47\frac{28}{30}$	6,350

December

Month.	Barometer.	State of the Weather and Wind.	Fahrenheit's Thermom.		Ombr.
				Med.	Inches.
December	Higheſt 1 30,36 Loweſt 23 28,44	Stormy and rainy for the moſt part; reſt hazy, ſhowery, cloudy with a few calms. A ſtorm the 22d and 23d. N.B. from the 22d, 9 P. M. to the 23d, 7 A. M (viz. in ten hours) the Barometer fell 72°; and on the 23d at noon was down to 28,44 i. e. very low for this ſtation. Wind 23 days from the Weſt, reſt equal.	Higheſt 10 53 Loweſt 1 38	$46\frac{26\frac{1}{2}}{31}$	4,190

Total Rain this Year 1769, $42,\frac{730}{1000}$

Received February 7, 1770.

XXII. *A Letter to Dr.* William Watſon, *F. R. S. giving ſome Account of the Manna Tree, and of the Tarantula: By* Dominico Cirillo, *M. D. Profeſſor of Natural Hiſtory at the Univerſity of* Naples.

S I R,

London, Feb. 4, 1770.

Read April 26, 1770.

As ſome natural productions of our warm Neapolitan climate ſeemed to engage your curioſity, to know the particular facts relating to ſome of them, and as I had an opportunity of examining every ſpot of the kingdom of Naples and Sicily, in the year 1766; I ſhall try to communicate to you the obſervations I made upon two very curious and intereſting ſubjects, *viz.* the Manna tree and the Tarantula. My misfortune is, that I have not my papers with me, which would have enabled me to give you a more full and ſatisfactory account; but, however, I flatter myſelf, I ſhall be able to trace out what is more eſſential and material to the purpoſe.

The Manna tree, commonly called *Ornus* by the botaniſts, is a kind of aſh tree, and is to be found under the name of *Fraxinus Ornus*, in Linneus' Sp. Plant. I ſhall ſay but very little concerning the botanic diſcription of this tree, becauſe it has been given by all the writers; and I ſhall only obſerve, that this kind of *fraxinus* is very eaſily diſtinguiſhed from the common *fraxinus* ſive *fraxinus excelſior*, by the leaves, which are round at the top, *ſubrotunda*, *integerrima*. This tree very ſeldom grows to a conſiderable height, nor does it acquire a conſiderable bulk; in general it is from 10 to 20 feet high, the trunk is commonly of 5 or 6 inches in diameter, and the branches are pretty numerous, and irregularly ſpread: theſe dimenſions, however, vary, if theſe trees are not crowded together, and have more liberty of growth. The Manna tree is common, not only in Calabria and Sicily, but alſo on the famous mountain Garganus, ſituated near the old town of Sypontum upon the Adriatic; and is mentioned even by Horace as an inhabitant of that mountain,

> " Aut Aquilonibus querceta Gargani laborant
> " Aut foliis viduantur Orni.

In all the woods near Naples the Manna tree is to be found very often; but, for want of cultivation, it never produces any manna, and is rather a ſhrub than a tree. The manner, in which the manna is obtained from the Ornus, though very ſimple, has been yet very much miſunderſtood by all thoſe who travelled in the kingdom of Naples; and among other things they ſeem to agree, that the beſt and pureſt manna is obtained from the leaves of the tree; but this, I believe, is an opinion taken from the doctrine of the antients, and received as an inconteſtable obſervation, without conſulting nature. I never ſaw ſuch a kind, and all thoſe who are employed in the gathering of the manna, know of none that comes from the leaves. The manna is generally of two kinds; not on account of the intrinſic quality of them being different, but only becauſe they are got in a different manner. In order to have the manna, thoſe who have the management of the woods of the Orni in the month of July and Auguſt, when the weather is very dry and warm, make an oblong inciſion, and take off from the bark of the tree about three inches in length, and two in breadth; they leave the wound open, and by degrees the manna runs out, and is almoſt ſuddenly thickened to its proper conſiſtence, and is found adhering to the bark of the tree. This manna which is collected in baſkets, and goes under the name of *manna graſſa*, is put in a dry place, becauſe moiſt and wet places will ſoon diſſolve it again. This firſt kind is often in large irregular pieces of a browniſh colour, and frequently is full of duſt and other impurities. But when the people want to have a very fine manna, they apply to the inciſion of the bark, thin ſtraw, or ſmall bits of ſhrubs, ſo that the manna, in coming out, runs upon thoſe bodies, and is collected in a ſort of regular tubes, which give it the name of *manna in cannoli*, that is, manna in tubes: this ſecond kind is more eſteemed, and always preferred to the other, becauſe it is free and clear. There is indeed a third kind of manna, which is not commonly to be met with, and which I have ſeen after I left Calabria: it is very white, like ſugar; but as it is rather for curioſity than for uſe, I ſhall ſay no more of it. The two ſorts of manna already mentioned undergo no kind of preparation whatſoever, before they are exported; ſometimes they are finer, particularly the *manna graſſa*, and ſometimes very dirty and full of impurities; but the Neapolitans have no intereſt in adulterating the manna, becauſe they always have a great deal more than what they generally export; and if manna is kept in the magazines, it receives often very great hurt by the Southern winds, ſo common in our part of the world. The changes of the weather produce a ſudden alteration in the time that the manna is to be gathered; and, for this reaſon, when the ſummer is rainy, the manna is always very ſcarce and very bad.

With regard to the uſe we make of manna in the practice of phyſic, I believe it is of very little conſequence; for it cannot be employed alone as a cathartic, becauſe you muſt give a conſiderable doſe in order to obtain a tolerable operation; it is commonly preſcribed for children, who ſooner take it becauſe it is ſweet, and ſometimes is given in colds and coughs: the generality of the phyſicians at Naples often give manna and ſalts to keep the body open in the beginning of many fevers, in which there is a foulneſs of the *primæ viæ*. We do not give any preference to the manna, in any particular caſe, and rather conſider it as an article of trade than a very uſeful medicine.

After this ſhort account of the manna, according to my promiſe, I ſhall give you a little of the hiſtory of the Tarantula, becauſe I have had an opportunity of examining the effects of this animal, in the province of Taranto, where it is found in great abundance: but I am afraid I ſhall have nothing more to ſay, than that the ſurprizing cure of the bite of the Tarantula, by muſic, has not the leaſt truth in it; and that it is only an invention of the people, who want to get a little money, by dancing when they ſay the tarantiſm begins. I make no doubt but ſometimes the heat of the climate contributes very much to warm their imagination, and to throw them into a delirium, which may be in ſome meaſure cured by muſic: but ſeveral experiments have been tried with the Tarantula; and neither men nor animals, after the bite, have had any other complaint, but a very trifling inflammation upon the part, like thoſe produced by the bite of a ſcorpion, which go off by themſelves without any danger at all. In Sicily, where the ſummer is ſtill warmer than in any part of the kingdom of Naples, the Tarantula is never dangerous, and muſic is never employed for the cure of the pretended tarantiſm. It is no doubt very extraordinary, that a man of ſenſe, and a phyſician of great learning, as Baglivi, ſhould have been ſatisfied with the account of this diſorder; and that, inſtead of examining the fact by experiments, he ſhould rather have tried to explain it: but even philoſophers like very much to meet with wonderful and extraordinary things, and though they are againſt all reaſon, ſtill they want them to be true, and endeavour to find out the cauſe of them. Every year this ſurprizing diſorder loſes ground; and I am perſuaded, that in a very little while it will entirely loſe its credit. The Neapolitan phyſicians all look upon the Tarantula in the ſame light, particularly after the ingenious book publiſhed on this ſubject, by the learned Dr. Serao, who, by various experiments, has proved, that the bite of the Tarantula never produced any bad effects, and that muſic never had any thing to do with it. The natural hiſtory and the deſcription of this ſpider is ſo well known, that I think it quite unneceſſary to enter into any farther particulars relating to it.

I hope I ſhall be able to ſend you, in a few months, ſome of my obſervations upon mount Ætna, and ſeveral curious things concerning the natural hiſtory, both of Sicily and Calabria; I am, in the mean time,

Your moſt obedient

humble ſervant,

Dominico Cirillo.

XXIII. *Obſervations made at* Dinapoor, June 4, 1769, *on the Planet* Venus, *when paſſing over the* Sun's *Diſk*, June 4, 1769, *with Three different Quadrants, and a Two Foot reflecting Teleſcope: Communicated to the Royal Society, by the Court of Directors of the* Eaſt India *Company.*

	h ′ ″		° ′ ″
At ſun-riſe cloudy			
At	5 20 32 A.M.	The ſun diſingaged from the clouds when his true altitude was	1 12 11
		When Venus appeared on the ſun's diſk	
At	7 5 22	The beginning of the emerſion when the ſun's altitude was	23 35 57
At	7 23 36	The end of the emerſion when the ſun's altitude was	27 29 20
		The ſun's meridian altitude this day, was	87 56
		The latitude of the place where the obſervation was made, is 25° 27′ N°.	

The above obſervation was made by Luis Degloſs, Captain of engineers, with the aſſiſtance of J. Lang and A. Stoker.

N. B. The ſun's altitude with the hour is exactly corrected, and all the allowances made.

XXIV. *Directions for making a Machine for finding the Roots of Equations univerſally, with the Manner of uſing it: By the Rev. Mr.* Rowning, *to* John Bevis, *M. D. F. R. S.*

S I R,

Read May 3, 1770.

PERUSING a diſcourſe in the memoirs of the Royal Academy at Peterſburgh, Tome vii. page 211, by the learned John Andrew de Segner, containing an univerſal method of diſcovering the roots of equations, which you was ſo kind as to recommend to my conſideration; I found, that the author's method, as you obſerved to me, conſiſted in finding ſeveral ordinates of a parabolic curve, ſuch, that its abſciſſas being taken equal to any aſſumed values of the unknown quantity in the equation, the ordinates correſponding to thoſe abſciſſas, ſhould be equal to the values of all the terms in the equation (when brought to one ſide) that is, in other words, in finding ſeveral ordinates of a parabolic curve defined by the equation propoſed: in which caſe, as is well known, if a curve be drawn through the extremities of the ſaid ordinates, the points upon the axis, where the curve ſhall cut it, will neceſſarily give the ſeveral values of the *real* roots

Fig. 1

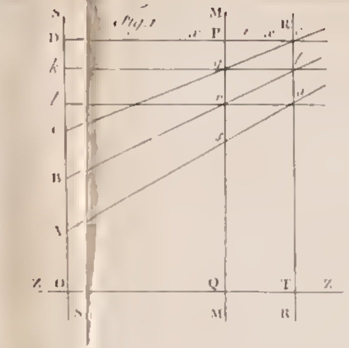


Fig. 2

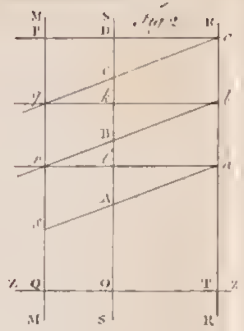


Fig. 3

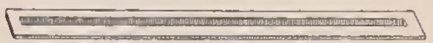
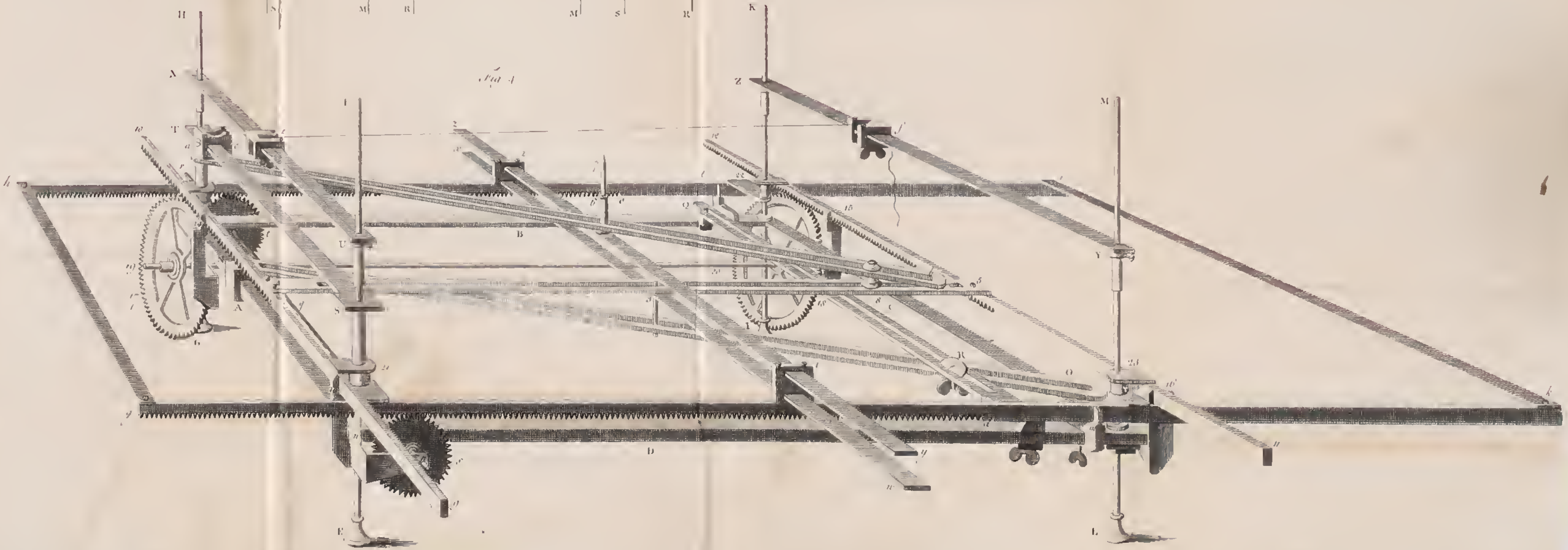


Fig. 4



[illegible] sculp.

roots of the equation; and the ſeveral points, where the curve ſhall approach the baſe, but ſhall return without reaching it, will ſhew the *impoſſible* ones.

This is a method I myſelf fell into ten or twelve years ago, and have conſtantly uſed for finding the roots of ſuch equations as I have had occaſion to conſider. But his method is preferable to mine in one reſpect, *viz.* that whereas I always compute the value of the ordinates in numbers, he finds them by drawing certain right lines; however, when there are both poſſible and impoſſible roots in an equation, as generally there are, theſe methods are both of them extremely embarraſſing: the learned author therefore wiſhes, that ſome method might be thought of, whereby ſuch curves, as we are now ſpeaking of, might in all caſes be deſcribed by *local* motion; but this, he tells us, he looked upon as ſo very difficult a taſk, that he never attempted it. *Quod ad deſcriptionem attinet*, ſays he, *motum excogitare, quo tales accuratè deſignari poſſunt omnes* [*hujuſmodi curvæ*] *admodum difficile judico, quare id neque tentavi.* This hint, however, convinced me, that the thing was poſſible; I therefore determined to endeavour it.

I ſoon found, that if rulers were properly centered, and ſo combined together, that they ſhould always continue repreſentatives of the ſeveral right lines, by which he diſcovers the abovementioned ordinates, upon moving the firſt, a point or pencil, ſo fixed as to be carried along perpetually by the interſection of the firſt and laſt rulers, would deſcribe the required curve, let the number of dimenſions in the equation be what it will; only the greater that number, the greater muſt be the number of the rulers made uſe of. And this appeared to me ſo obvious, that I wondered, neither the learned author, who ſeems to have the thing much at heart, nor any body elſe ſince the publication thereof, ſaw it.

Let the equation to be reſolved, be $a+bx+cxx+dxxx$, &c.$=o$.

Upon the line ZZ, as a baſe, in either the firſt or ſecond figure, draw the lines SS and RR perpendicular thereto, at any diſtance taken at pleaſure from each other. Then upon the line SS in either figure, Tab. VIII. ſet off the lines OA, AB, BC, CD, &c. proportionable to the coefficients a, b, c, d, &c. in the equation, remembering to take each line upwards from the extremity of the laſt, when the coefficient, it is to repreſent, is *affirmative*; but downwards from the ſaid extremity, when the coefficient is *negative*. Then through the extremity of the laſt of the lines OA, AB, BC, &c. which in our caſe is D, draw the line Dc, parallel to the baſe ZZ, and through c, where Dc interſects RR, draw cC; and parallel to SS, and at any diſtance from it taken at pleaſure, draw MM: then parallel to Dc, and where Cc interſects MM, draw the line kb; through b, where this laſt line interſects RR, draw bB; parallel alſo to Dc, and where bB interſects MM, draw la; and through a, where la interſects RR, draw aA. This done, let SS, RR and Cc, be ſuppoſed to repreſent three rulers with grooves or notches cut quite through them, of ſuch form as is repreſented Fig. 3. and let thoſe rulers be ſcrewed down in their reſpective places SS, RR, and Cc, to a plane or frame of ſufficient ſize.

Then let other rulers of like form, as B*b*, A*a*, &c. be *moveable* upon the centers B, A, &c. and let thoſe centers themſelves be moveable upwards and downwards in the groove or notch in the ruler SS, and in ſuch manner, that the centers B and A may be placed upon one another, or upon C, if occaſion requires, and let thoſe centers be ſcrewed down in their proper places, *viz.* the center A at A, the center B at B, &c. Then let *kb* and *la* repreſent other moveable rulers, ſuch as the former, and ſo confined that they ſhall always continue parallel to themſelves and to the line D*c*; and let MM repreſent another ruler of like form. And let the rulers *kb* and MM be connected with the fixed ruler C*c* by a ſliding pin paſſing through the interſection of their notches at *q*, and let the rulers *kb*, B*b*, *la* and A*a* be connected with one another and with MM and RR, by like pins paſſing through their notches at *b*, *r*, *a*, and *s*; and let the laſt of theſe pins *s*, have the point of a pencil fixed therein; then, I ſay, that if the ruler MM be moved backwards or forwards, from or towards SS, and always held parallel thereto (or be ſo confined as to be capable of moving in no other manner) the pencil *s* ſhall deſcribe the required curve. And the diſtances from the point O, at which that pencil ſhall croſs the baſe ZZ, on the right hand ſide of SS, ſhall denote the *affirmative* roots of the equation; and the diſtances from the ſame point O, where it ſhall croſs the ſaid baſe on the left hand ſide of SS, ſhall expreſs the *negative* ones; and the places, where it ſhall approach the ſaid baſe, but ſhall return without reaching it, ſhall indicate the *impoſſible* ones; the ſaid diſtances being

always

always to be eſtimated on a ſcale on which the aſſumed line D c is *unity*.

Demonſt. Since the lines OA, AB, BC, &c. are to be taken *proportional* to the coefficients a, b, c, &c. let us ſuppoſe the firſt of them, *viz.* OA, to be taken equal to the firſt coefficient a, or to any part of it taken at pleaſure; ſuppoſe for inſtance, to the n$^{\text{th}}$ part, that is, to $\frac{a}{n}$; then, to preſerve the above-mentioned proportionality, the next *viz.* AB will be equal to $\frac{b}{n}$, BC will be equal to $\frac{c}{n}$, and CD to $\frac{d}{n}$, &c. Call OQ or its equal DP, x; then Dc, as above-mentioned, being taken equal to *unity*, Pc will be equal to $1-x$, and DC being equal to $\frac{d}{n}$, and the triangles DCc and Pqe being ſimilar, we have this proportion, *viz.* $1 : 1-x :: \frac{d}{n} : \frac{d-dx}{n} =$ Pq or Dk; but kB is equal BC+CD—Dk, that is, to $\frac{c}{n}+\frac{d}{n}-\frac{d-dx}{n}$, that is, to $\frac{c+dx}{n}$; and by ſimilar triangles, as $kb : qb :: k\text{B} : qr$, that is, in ſymbols, as $1 : 1-x :: \frac{c+dx}{n}$; $\frac{c+dx-cx-dxx}{n} = qr$ or kl: but Al is equal to AD—Dk—kl, that is, in ſymbols, to $\frac{b}{n}+\frac{c}{n}+\frac{d}{n}-\frac{d-dx}{n}-\frac{c+dx-cx-dxx}{n}$, that is, to $\frac{b+cx+dxx}{n}$; and by ſimilar triangles, $la : ra :: \text{A}l : rs$; in ſymbols $1 : 1-x :: \frac{b+cx+dxx}{n} : \frac{b+cx+dxx-bx-cxx-dxxx}{n} = rs$; Q$s$ therefore, which by the figure is equal to

$QP - Pq - qr - rs$, is equal to $\frac{a+b+c+d-d-dx}{n}$ $- \frac{c+dx-cx-dxx}{n} - \frac{b+cx+dxx-bx-cxx-dxxx}{n}$, that is, to $\frac{a+bx+cxx+dxxx}{n}$; conſequently, when Qs is nothing, that is, when the curve deſcribed by s, cuts the baſe, $\frac{a+bx+cxx+dx^3}{n}$ will be equal to nothing, and therefore equal alſo to $a+bx+cxx+dxxx$; this laſt being alſo, from the equation propoſed, equal to nothing; Qs therefore in thoſe circumſtances will be equal to $a+bx+cxx+dxxx$; and conſequently whatever value of x or OQ renders $a+bx+cxx+dxxx$ equal to nothing, will render Qs equal to nothing: but every value of x that renders $a+bx+cxx+dxxx$ equal to nothing, is a root of the propoſed equation $a+bx+cxx+dxxx=0$; conſequently the curve will croſs the baſe ZZ at every *real* root of that equation, whether negative or affirmative, and therefore, as every one acquainted with curve lines knows, will attempt to do ſo, but not quite reach it, at every *impoſſible* one. Q, E. D.

This demonſtration is adapted only to an equation of three dimenſions; but it is eaſy to ſee, it may be extended to any other.

Note. To obtain the negative roots, the rulers muſt be extended to the left of the line SS, as repreſented fig. 2. where they are denoted by the ſame letters as in the other figure; *viz.* the ruler Cc muſt be extended from c to q; the ruler Bb from b to r, and aA from a to s, and onwards towards the left, the two laſt turning upon the centers A, B, in the fixed line SS.

It is not neceſſary, that the curve ſhould be deſcribed with accuracy, or even that it ſhould fall upon the plane or frame, except where it croſſes or attempts to croſs the baſe; and therefore there will ariſe no inconveniency in this reſpect from taking the lines OA, AB, &c. large. But the immoveable rulers OD and Tc muſt be fixed ſo near together, that, their diſtance Dc or OT, being reckoned *unity*, the baſe OT extended to the extremity of the plane to the right, may take in all the *affirmative* roots: and when extended to the left, may take in all the *negative* ones.

There is one thing more, very well worth obſerving, *viz.* that if you have an equation of this ſort $xxx - 5xx + 1200x + 9000 = 0$ (where you may obſerve, that the coefficients unity, 5, 1200 and 9000 are ſo very different from each other, that it would be difficult to ſet them off in due proportion upon the line OD) you may reduce them into a more manageable form, by the following contrivance: inſtead of each x in the equation put $10x$, $20x$ or $100x$, ſuppoſe $20x$; then inſtead of xxx you will have $8000xxx$, inſtead of $-5xx$ you will have $-2000xx$, &c. and the whole equation will ſtand thus, $8000xxx - 2000xxx + 24000x + 9000 = 0$; then divide each term by 100, and you bring it to $8xxx - 2xx + 24x + 9 = 0$, a much more tractable equation than the other: but then it muſt be remembered, that as you made x 20 times leſs than it was, the roots when diſcovered will be ſo many times leſs than the true ones, and therefore each muſt be multiplied by that number 20.

The following obſervations relative to the application of the abovementioned rulers may be uſeful:

1. The roots of equation are of three ſorts, *affirmative*, *negative*, and *impoſſible*; which laſt are ſometimes called *imaginary*.

2. Every equation contains as many roots as it has dimenſions.

3. The impoſſible roots go by pairs. Thus, if an equation has an impoſſible root of this form $a+b\sqrt{-1}$, it has another impoſſible one of this, *viz.* $a-b\sqrt{-1}$, which may be called its fellow: from hence it follows, that every equation, if it has any impoſſible roots, has either *two*, *four*, or *ſix*, &c. that is, an even number of them: and every time the curve deſcribed by the rulers abovementioned approaches the baſe, and leaves it again without croſſing it, it implies one impoſſible root and its fellow; ſo that if it approaches the baſe in this manner three times, it implies, that the equation has ſix impoſſible roots. And this is all the rulers can do with regard to this ſort of roots; they cannot ſhew *what* thoſe are, but only *how many* there are. I ſhall direct to a method of finding what they are in the 8th article below. Inaſmuch then, as the impoſſible roots go by pairs, and the number of roots in any equation is equal to the number of its dimenſions, it follows,

4. That every equation of an odd number of dimenſions, muſt contain at leaſt one *real* root.

5. Every equation whoſe firſt and laſt terms (when brought to one ſide) have contrary ſigns, will have at leaſt alſo one real root: and therefore, when this is the caſe, and the number of its dimenſions is

alſo *even*, it muſt have two real roots at leaſt: becauſe the number of dimenſions being *even*, and the number of impoſſible roots always *even*, the number of real ones muſt be *even* alſo.

6. If any equation be divided by the unknown quantity *minus* one of its roots, it will be reduced one dimenſion lower. And as every equation contains as many roots as it has dimenſions, it follows,

7. That if you deduct the number of impoſſible roots from the whole number of its roots, that is, from the number of its dimenſions, the remainder will give the number of its real roots.

8. When you have found by the rulers, what thoſe real roots are, put the unknown quantity (x) equal to each of them, tranſpoſe the terms in each equation to one ſide, multiply all the equations together, and divide the equation propoſed by their product; then make the quotient equal to nothing, and you have an equation containing all your impoſſible roots, without any real ones intermixed. Then thoſe impoſſible roots may be found by the method for that purpoſe laid down by Monſ. de *Bougainville* in his *Traité du Calcul integral,* in the fifth and ſixth chapters of his introduction; and which is the beſt method I know of.

His method conſiſts in parting the equation into two others, of the ſame number of dimenſions indeed, but ſuch as ſhall involve no other than real roots; which real roots you may then find by theſe rulers, or otherwiſe; and from thence you will obtain all the impoſſible roots of your equation. But becauſe few Engliſh mathematicians, I ſuſpect, are acquainted with this method, it may be uſeful to give the ſubſtance of it here in our own language.

The author previouſly demonſtrates the two following proportions.

PROP. I. That when a quantity is equal to nothing, and is compoſed of many terms, ſome of which are *real*, and the other are terms *multiplied by* $\sqrt{-1}$, the ſum of the real ones is equal to nothing; and the ſum of thoſe that are multiplied by $\sqrt{-1}$ is alſo equal to nothing. This is the 69th article of his introduction.

PROP. II. That when an equation involves imaginary roots only, the unknown quantity may always be ſuppoſed equal to $m+n\sqrt{-1}$; m and n being real quantities. This is the 80th article of his introduction.

Then to find the roots of ſuch an equation as we are ſpeaking of, for every unknown quantity (x ſuppoſe) in the equation, ſubſtitute $m+n\sqrt{-1}$, and you will obtain a new equation involving real terms, and terms multiplied by $\sqrt{-1}$; the former of which by Prop. I. are always equal to nothing, and ſo are the latter: make them ſo therefore, and you have two equations, from which the two aſſumed quantities m and n may be diſcovered; and conſequently, ſince the value of x is by the ſecond propoſition equal to $m+n\sqrt{-1}$, it is diſcovered alſo.

What I mean in the former part of this article may be explained by the following inſtance; ſuppoſe the real roots diſcovered by the rulers abovementioned to be a, b, $-c$, &c. then put $x=a$, $x=b$, $x=-c$, &c. tranſpoſe the terms to one ſide, and you have $x-a=0$, $x-b=0$, $x+c=0$, &c. multiply all theſe laſt equations together, divide the equation propoſed by their product, and proceed as abovementioned.

9. The greateſt negative coefficient of any equation (conſidered as affirmative) and increaſed by unity always exceeds the greateſt affirmative root of the equation. And therefore,

10. If for the unknown quantity (x) in the equation, you put that coefficient taken affirmatively and increaſed by unity minus x, all the roots of the equation will be rendered affirmative. If you do this, you need only uſe ſuch rulers, as are deſcribed in the firſt figure, whoſe centers are at their extremities, and ſo one ſort of rulers will be ſufficient for all caſes. For you may obſerve, thoſe in the ſecond figure are different from the other, as to their centers.

11. If, when you have made all the roots of your equation affirmative, you would avoid removing the ruler MM to the right hand ſide of RR, which might be attended with inconveniency; that is, if you would have all the roots of your equation fall between O and T, that is, between nothing and unity, inſtead of the unknown quantity x in your laſt equation, put x multiplied by the greateſt negative coefficient therein conſidered as affirmative and increaſed by unity; for inſtance, if the greateſt negative coefficient in the equation be minus 9, put $10x$ inſtead of every x in the equation, and you will have a new equation, all whoſe roots ſhall fall upon the line OT unproduced; for then they will be leſs than unity, that is, than De~~C~~ or OT: but when the roots are thus found, each of them muſt be multiplied by that coefficient increaſed by unity, that is, in the above inſtance, by 10; becauſe putting $10x$

for x, is making every root ten times ſmaller than it is.

Theſe propoſitions are all taken from the writings of the algebraiſts, or however are ſuch as eaſily follow from them, and therefore need not be demonſtrated here.

The following is a deſcription of a machine for regulating the motion of the abovementioned rulers, which I cauſed to be made by an excellent workman in this town, and which I deſire the ſociety to accept of, to be kept as a ſpecimen, for the inſpection of any gentleman, who may chuſe to have ſuch made. It extends only to equations of two dimenſions; but it is eaſy to ſee from it, how it may be carried to others of any number. A draft of it is exhibited in fig. 4. of the abovementioned table: where ABCD repreſents a frame of iron or ſteel, conſiſting of four ſtrait bars joined together at their extremities, and forming a rectangular parallelogram about 12 inches long and 8 broad; into which at its four corners are ſcrewed four perpendicular columns EF, GH, IK and LM, whoſe lower ends ſerve it as feet to ſtand on. And on one of the aforeſaid bars, *viz.* on A, is a moveable nut or ſlider, which may be ſcrewed to it at any point thereof; it appears in the figure at N; and on this nut, as a center, one end of the bar NO turns, whoſe other end is ſcrewed down to the croſs bar PQ at R, which croſs bar is ſcrewed down to the frame at P and Q, and may be ſet nearer or further from the end A at pleaſure: this bar repreſents the line RR in fig. 1. Then on the perpendicular pillars E F, G H, I K and L M, are fixed three bars S T, U X and Y Z; on the firſt of theſe S T, is a ſliding nut *c*, which carries one extremity of the bar *a b* turning upon it as a center; on the ſecond and third, *viz.* U X and Y Z, are alſo two nuts *e* and *f*, which may be ſcrewed on any part of them, and on which the ſilken cord *e f* is faſtned. The two firſt of thoſe bars, *viz.* S T and U X together with the bar A, or rather lines ſuch as the pricked line upon the upper one drawn upon them from pillar to pillar, repreſent the line SS in fig. 1. and the ſilken cord extended from the nut *e* to *f* and fixed to them, repreſents the baſe line Z Z in that figure.

Then beſides theſe there is another rectangular figure *g h i k*, about twice the length of the former, whoſe ſides, *g k* and *hi* ſlide in grooves or ſupporters ſcrewed to the frame ABCD, at proper places (three of which may be ſeen at *l*, *m*, and *n*), and have triangular teeth on their under ſides from *g* to *d*, and from *h* to *o*, which run in ſimilar teeth in two wheels *s* and *t*, of equal diameters, fixed on an axis *p q r*, and their axis *p r* is ſupported by proper cocks, one of which appears at *u*, the other not ſeen in this view of the figure. Theſe wheels being both fixed to the ſame axis, and the triangular teeth in the bars fitting thoſe in the wheels exactly, occaſion the bars *g k* and *h i* to move with equal paces, when the machine is put in motion; by which means the bars *wx* and *yz*, which ſlide in two pieces 1 and 2 ſcrewed to the ſaid bars *g k* and *hi*, neceſſarily move parallel to themſelves: theſe bars repreſent M M in fig. 1. In the lower of theſe bars *w x* is fixed a perpendicular pin, at 3, whoſe upper end paſſes through the groove of the bar 4, 5, and its lower end through that of the bar N O, and in the upper of theſe bars *y z*, is fixed a perpendicular pin 6, 7, the upper end of which may be taken out, and a pencil put in its place; this pin repreſents the point *s*, and the former pin 3, the point *r*, in fig. 1. There is alſo a perpendicular pin ſcrewed to the bar 4, 5, at 8, which muſt be fixed upon that bar directly over the groove in the bar PQ. This pin denotes the point *a* in fig. 1. There are alſo two bars 9, 10, and 11, 12, with triangular teeth, which bars ſlide in ſupporters ſcrewed to the frame at proper places (which appear at 13, 14, 15, and 16), and ride upon the wheels 17 and 18, with ſimilar teeth, and both fixed on the axis 19, 20. Theſe wheels prevent the ſaid bars from moving with unequal paces, and therefore cauſe the bar 4, 5, which is ſcrewed down to them at each end of it, to move parallel to itſelf; this bar repreſents *l a* in fig. 1. Then the aforeſaid nuts or ſliders *e*, *f*, *c*, N, and R, being ſcrewed down at proper places according to the coefficients of the equation (as ſhall be directed more particularly in the next article), and the bar *g h* being puſhed forwards or backwards by the hand, will put the whole in motion; and the pin 6, 7, will deſcribe a curve, which ſhall be the *locus* of the equation, and the diſtances where it ſhall paſs under the ſilken cord *e f*, reckoning from the pricked line upon the bar U X, ſhall denote its real roots; and as many times as that pin ſhall approach the ſaid cord, and then recede from it without paſſing under it, twice ſo many will be the number of impoſſible roots in that equation. N. B. There are ſome ſmall pieces ſcrewed on to the perpendicular pillars E F, G H, I K and L M, three of which appear at 21, 22, and 23; theſe ſerve only to prevent the bars which ſlide under them from riſing up.

To rectify the machine for a propoſed equation.

Screw the nuts *e* and *f*, which carry the ſilken cord at any equal diſtances from the pillars E F and L M. Then ſlide the nut *c*, which carries the extremity of the bar *a b*, till it is farther from the pillar E F than the place you have fixed the nut *e* at, by a number of diviſions upon any ſcale of equal parts, equal to the known term of the equation, if that term be affirmative; but ſo much nearer to it, if that term be negative; and fix it there. Then ſlide the nut N that carries the bar N O, till it is farther from or nearer to the pillar E F, than the laſt nut *c* is, by a number of diviſions taken from the ſame ſeale, equal to the ſecond coefficient of the equation (I mean that, where the unknown quantity is of one dimenſion); farther from it, if that coefficient is affirmative; but nearer, if it is negative: and fix it there. Then, laſtly, ſlide the nut R, which fixes the other end of the bar N O, till it is farther from a line drawn from the pillar E F to L M, that is, farther from the ſide D of the frame than the laſt nut N is from the ſame, as many diviſions, as the coefficient of that term of the equation, where the unknown quantity is of two dimenſions, indicates; farther, if that coefficient is affirmative, otherwiſe nearer to it: for which purpoſe the end A of the frame, and the bars ST, UX, YZ and the croſs bar PQ ſhould all be graduated, beginning at the front of the frame D. They are graduated on the machine itſelf in a manner ſomewhat different, but it is found not ſo convenient for uſe. Then the ſeveral diſtances upon the cord *ef*, where the pencil or pin 6, 7, ſhall croſs it, being reckoned from the pricked line upon the upper bar UX, and meaſured upon a ſcale, on which the diſtance of the croſs bar PQ from a line drawn along the middle of the end A, from EF to GH, is *unity* (the reaſon of which appears from the foregoing demonſtration, where D*c* or OT in fig. 1. and which anſwers to the diſtance of this line PQ from the bar A, was put equal to *unity)* ſhall give the required roots. And if the cord *ef* is removed, and a piece of paſteboard put over the machine, on the two upper bars UX and YZ, having a ſtrait line drawn on its under ſide, repreſenting the cord *ef*, and a pencil with its point upwards be put in the place of the pin 7, that pencil will deſcribe on the under ſide of the paſteboard a curve, which with the ſaid right line ſhall *conſtruct* the equation propoſed: and the larger the coefficients of the equation are (which coefficients may be made as large as you pleaſe without altering the roots, by multiplying them by any number at pleaſure) the larger will be the angles at which the curve and the ſtrait line ſhall cut each other; which in the conſtruction of equations is a very deſireable circumſtance. And as it appears from the foregoing demonſtration, that this machine by the application of more bars, may be extended to equations of *all dimenſions*, it may not, I think, be improperly called an *Univerſal conſtructor of Equations*. I am

SIR,

With great reſpect,

your moſt humble and

moſt obedient ſervant,

London, Eſſex-ſtreet,
March 24, 1768.

J. Rowning.

Received March 15, 1770.

XXIII.* *On the late Tranſit of* Venus: *By* Nathan Pigott, *Eſq*; *to Dr.* Bevis, *F. R. S.*

Caen, Lower Normandy, Feb. 9th, 1770.

Dear Sir,

Read April 26, 1770.

I TAKE the opportunity of Capt. Hue's ſailing from this place, to ſend you the obſervations of the laſt tranſit of Venus over the Sun, made here, and which I would have remitted ſooner, if an opportunity had offered. I ſhould be obliged to you, if you would communicate theſe obſervations to the Royal Society. I have added thoſe of other obſervers, which have been ſent to me, from different places, and reduced the ſame to the obſervatory of Paris, keeping an account only of the difference of meridians, as inſerted in the *Connoiſſance des tems*, and omitting the ſmall correction of the parallax, ſuitable to the different ſituation of theſe places, becauſe, I preſume, the longitude of ſome of them, is not known with ſufficient preciſion, to admit here of this very ſmall equation.

TABLE of OBSERVATIONS reduced to the Meridian of the R. Obſervatory, at Paris.

Places of obſervations	Obſervers	External contact	Internal contact	Focus of inſtruments	Mag. powers	External contact reduced to Paris	Internal contact reduced to Paris
	Meſſ.	h ′ ″	h ′ ″			h ′ ″	h ′ ″
Greenwich	Maſkeline	7 10 55	7 29 23	teleſcope 2 feet	140	7 20 11	7 38 39
	Hitchens	7 10 54	7 28 57	teleſcope 6 feet	90	7 20 10	7 38 13
	Hirſt	7 11 11	7 29 18	teleſcope 2 feet	55	7 20 27	7 38 34
	Horſeley	7 10 44	7 29 28	achro. 10 feet	50	7 20 00	7 38 44
	Dun	7 10 37	7 29 48	achro. 3½ feet	140	7 19 53	7 39 4
	Dollond	7 11 19	7 29 20	achro. 3½ feet	150	7 20 35	7 38 36
	Nairne	7 11 30	7 29 20	teleſcope 2 feet	120	7 20 46	7 38 36
Kew	Bevis	7 10 2	7 28 17	teleſcope 3½ feet	120	7 20 27	7 38 42
Caen		7 9 20	7 27 43	teleſcope 12 inches		7 20 7	7 38 39
Obſ. of Paris							7 38 11,5
Ditto	Duke of Chaulnes						7 38 58
Paris	Meſſier						7 38 45
	Baudouin						7 38 51
	Turgot						7 38 50
	Zanoni						7 38 41
St. Hubert	Le Monier		7 34 56	achro. 10½ feet			7 36 52,5
Ditto	Chabert		7 35 32	refractor 18 feet			7 37 28,5
Miſſion	Rochefort		7 27 7,5	achro. 3 feet			7 37 54,5
	My Son		7 26 55,5	teleſcope 18 inches	55		7 37 42,5
	Self	7 9 38,5	7 26 24,5	achro. 6 feet	80	7 20 25,5	7 37 11,5

The place at Kew, where you obſerved, being 1′ 9″ to the Weſt of the obſervatory at Greenwich, is of courſe 10′ 25″ Weſt of that at Paris.

The obſervations in the table, joined by a ſtroke of the pen, were made together in the ſame place. You will perceive, in comparing the obſervations of the internal contact, how little the five laſt agree with the others. I am wholly ignorant in what light, the able aſtronomers, who obſerved at St. Hubert, conſider their obſervations.

Monſieur le Monnier, in communicating them to me, adds no remarks, no more than in the letters he has wrote to me ſince. I come therefore to thoſe made at the houſe called *La Miſſion*, ſituated in the neighbourhood of *Allemagne*, a village near this town. This houſe is about 500 toiſes South-Eaſt of my obſervatory at Caen, and their difference of meridians about 200 toiſes. I preferred this ſituation, from whence I could ſee the ſun almoſt in the horizon, to that of my obſervatory; the adjacent houſes of which, I foreſaw, would deprive me of the ſight of it, ſoon after the ingreſs of Venus.

June the firſt, I ſent to the abovementioned houſe a ſix feet achromatic refractor, with its micrometer made by Dollond, magnifying 90 times: an excellent teleſcope of 18 inches focus magnifying from 55 to 200 times made by Short, and a clock.

June 2d, I took twenty altitudes of the ſun, in the morning, to determine the going of a good compound pendulum, made by Le Paute, at Paris, which was to remain in the obſervatory at Caen, and to give the true times of the obſervations of the next day; and from correſponding ones in the afternoon, I concluded by a mean, which agreed with the reſt to leſs than one ſecond, that it was ſlow on mean time at noon 2′ 21″,8; and that ſince the 25th of May, it had loſt at the rate of 3″ a day on the mean motion of the ſun.

I then went to the *Miſſion*, to prepare for the operations of the next day; it had been more or leſs rainy from ſun riſing; but about ſix in the evening, the clouds diſperſed and the ſun ſet in the moſt auſpicious manner.

June 3d, I took the following correſponding altitudes of the ſun's ſuperior limb; light clouds being troubleſome.

A. M.			Alt.	P. M.			Noon by clock.		
h	′	″		h	′	″	h	′	″
8	43	20,0	44.00	15	7	25,0	11	55	22,5
	46	35,0	44.30		4	12,0		55	23,5
	48	12,0	44.45		2	33,0		55	22,5
	49	45,0	45.00		1	00,0		55	22,5
	51	24,0	45.15	14	59	23,0		55	23,5
	53	3,0	45.30		57	42,0		55	22,5
	57	55,0	46.15		52	49,0		55	22,0
	59	33,0	46.30		51	12,0		55	22,5
9	1	13,0	46.45		49	32,0		55	22.5
				Mean			11	55	22,5+
				Correction				—	4,3
				Clock at true noon			11	55	18,2
				Mean time at ditto			11	57	43,7
				Clock ſlow on mean time				2	25,5

Theſe correſponding altitudes being taken, I went, as the day before, about four o'clock to *La Miſſion*; where I met Monſieur de Rochfort, a gentleman of

Caen, and my ſon. It rained by intervals, as it had done the whole day; the quickſilver in the barometer at noon being at 29,9 Engliſh inches, and Fahrenheit's thermometer in the obſervatory at 62°, the wind varying from the Weſt to the North.

I compared the going of the clock I had with me, with that which remained in the obſervatory by ſignals repeated five times, and which agreed perfectly together: the ſame was done after ſun ſet.

At half an hour paſt five, the clouds diſperſed entirely: the ſun ſhewed itſelf in all its ſplendor, and continued ſo the remaining part of the day.

About ſeven we all placed ourſelves at our inſtruments; Monſieur de Rochfort made uſe of a three feet achromatic refractor: my ſon of the 18 inch refractor: and I of the ſix feet achromatic refractor made by Dollond, and at $7^{h}\ 4'\ 58'',5$ of the clock, or $7^{h}\ 9'\ 38'',5$ apparent time, I perceived the external contacts of the Sun's and Venus's limbs.

As the impreſſion on the Sun's limb ſeemed conſiderable when I perceived it, I concluded this obſervation too late, which I judged to be occaſioned by a motion of undulation, with which the ſun was ſtrongly affected; for this reaſon, in two letters wrote to Paris the 11th of June and the 14th of July, and communicated to the Academy of Sciences there, I do not heſitate to declare this obſervation inſufficient; it agrees neverthelefs very well with yours made at Kew, and is nearly a mean between thoſe of Greenwich, as may be ſeen by the table. However, I prepared myſelf with all poſſible care, for the obſervation of the internal contacts; and though the Sun's limb moved continually up and down with a quick motion, I judged the internal contacts at 7h 21′ 44,5″ by the clock, or 7h 26′ 24,5″ apparent time, and 3″ or 4″ later, I ſaw a thread of light ſeparate the planet from the Sun,

	h	′	″	
Internal contacts, by M. de Rochfort	7	27	7,5	ap. time
By my ſon	7	26	55,5	

I find, by my regiſter, that Monſieur de Rochfort judged his obſervation ſome ſeconds too late.

I perceived that Venus, before ſhe ſeparated from the Sun, was conſiderably ſtretched out towards his limb, which gave the planet nearly the form of a pear; and even after the ſeparation of the limbs, Venus was twelve or more ſeconds before ſhe reſumed her rotundity.

	Clock			Ap. time			
	h	′	″	h	′	″	
At	7	30	27,0	7	35	7,0	Venus quite round.
	7	38	25,0	7	43	5 0	Venus's limb indented.
	7	45	8,0	7	49	48,0	Venus of a very irregular form, and ſtrongly affected by an odd twiſting motion.

I endeavoured to take the difference of right aſcenſion of the Sun and Venus; but I would not make the former deſcribe the æquatorial wire of the micrometer.

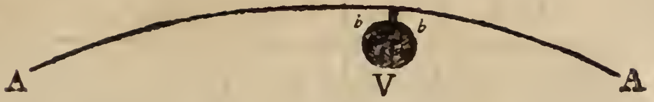
I cannot but repent having quitted my obſervatory, for the houſe in which I was; it is true indeed, as I have ſaid, that I had the advantage of ſeeing Venus till ſun ſet; but, in giving the preference to this ſituation, I unfortunately did not take notice, that I had

I had a river and a meadow between the Sun and me; the exhalations which riſe from ſuch places, eſpecially towards the evening, produced, no doubt, that undulatory motion of the Sun, which muſt render obſervations made in ſuch circumſtances more or leſs dubious.

In ſhort, I ſhall make two remarks on our obſervations: the firſt, that it ſeems odd, that the obſervation of the external contacts, which, for the reaſons beforementioned, I judged had been made too late, agrees nevertheleſs both with yours and thoſe of Greenwich: ſecondly, that although the internal contacts be marked by us ſooner than by any other obſervers, except thoſe of St. Hubert; we kept, nevertheleſs, the moſt ſcrupulous ſilence, nor did any motion indicate to the others, the times each wrote down; moreover, we were all three convinced that Venus and the Sun were ſeparated, when we began to count the clock. However, I can but conſider it as a misfortune, to have left my obſervatory, where the Sun's limb appeared perfectly well determined, even after the internal contacts, and not in the leaſt affected by any motion or undulation, which were ſo troubleſome to us; luckily I left a perſon in it, who did not neglect theſe favourable circumſtances; and the obſervations there made are the more intereſting, as the weather did not permit the external contacts to be obſerved, in any place from Caen to Breſt, no more than at Paris nor in its environs.

Obſervations of the contacts of the Sun and Venus, made in my obſervatory at Caen, with a 17 inch refractor with 2¾ inches aperture.

Clock			App. time			
h	′	″	h	′	″	
7	4	40,0	7	9	20,0	Sun well determined: a very ſmall impreſſion appeared on its ſuperior limb: it ſeemed even doubtful whether the contacts were formed.
7	4	52,0	7	9	32,0	The contacts very certain: this obſervation excellent, and it is thought the contacts could not have been ſeen ſooner than $7^h\,9'\,20''$.
7	20	33,0	7	25	13,0	The following limb of Venus ſeemed to touch that of the Sun: the planet appeared quite round; but ſoon after ſeemed to ſtretch itſelf out, and to form the tail, mentioned underneath: this obſervation is thought leſs certain than the others.
7	23	3,0	7	27	43,0	Internal contacts: by internal contacts muſt be underſtood the inſtant, when a ſort of tail, ſuch as is repreſented in the figure, and which joined Venus to the Sun's limb, ſeparated from it ſo ſuddenly, that it is impoſſible there could have been an error of one ſecond. There appeared inſtantly a conſiderable diſtance between the limbs; that diſtance was not meaſured, but it might be $\frac{1}{20}$ of the Sun's diameter; and that diſtance was concluded from the compariſon of the apparent length of this tail, to the diameter of Venus.



A A repreſents the Sun's ſuperior limb.

V Venus entirely upon the Sun only joined to his limb by the tail *b b*, which decreaſed in breadth till the inſtant of ſeparation: the length of this tail ſeemed equal to $\frac{1}{\text{[illegible]}}$ of Venus's diameter.

The calculation of this tranſit of Venus over the Sun, was made from tables corrected by that of the 6th June 1761; and, compared to obſervation, give the following differences.

	Calculation			Obſervation			Difference	
	h	′	″	h	′	″	′	″
External contacts	7	3	33,0	7	9	20,0	+ 5	47
Internal contacts	7	22	27,0	7	27	43,0	+ 5	16
Interval of contacts	0	18	54,0	0	18	23 0	− 0	31

June 4th light clouds flying and Fahrenheit's Thermometer at 60° I took the following equal altitudes of ⊙ ſuperior limb.

A. M.			Alt.		P. M.	Noon by clock
h	′	″	°	′		
9	44	6 0	53	00	Cloudy	
	45	58,0	53	15		
	47	49 0	53	30		
	49	40,0	53	4		
	51	36.0	54	00		
	53	29.0	54	15		
June 5th cloudy						

June 6th, I took the following altitudes of Sun's ſuperior limb.

A. M.			Alt.		P. M.	Noon by clock
h	′	″	°	′		
8	53	27,0	45	45		
	58	18,5	46	30		
	59	57,5	46	45		
9	1	38,5	47	00		
	4	53,5	47	30		
	8	14,5	48	00	Cloudy	
	11	37,5	48	30		
	13	16,5	48	45		
	14	58,5	49	00		
	16	40,5	49	15		
	18	23,0	49	30		

June 7th, cloudy.

June 8th, ditto.

June 9th, Fahrenheit's thermometer at 63°; I took the following correſponding altitudes of the Sun s ſuperior limb. Weather hazy.

A. M.			Alt.		P. M.			Noon by clock		
h	′	″	°	′	h	′	″	h	′	″
9	10	38,5	48	30	14	48	41,0	11	56	9,75
	17	23,5	49	30		34	57,0		56	10,75
	19	5,0	49	45		33	16,0		56	10,5
	20	46,0	50	00		31	35,0		56	10,5
	58	46,0	55	15		53	32,0		56	9,0
Mean								11	56	10,1
Correction									—	2,7
Clock at true noon								11	56	7,4
Mean time at true noon								11	58	47,6
Clock ſlow on mean time									2	40,2
Ditto, June 3d									2	25,5
Loſt on mean time in 6 days										14,7
Hence loſt on ditto, per day										2,45

It was from the correſponding altitudes of the Sun, taken the 3d and 9th of June, that I got the apparent times of the obſervations made at Caen; theſe times may be eaſily verified, by the altitudes taken in the mornings of the 4th and 6th; but I can ſufficiently depend on the clock to diſpenſe with that trouble.

I return you, dear Sir, many thanks for your obſervations of the tranſit, which you was ſo kind to ſend me; it is the moſt circumſtantial and compleat of any I have ſeen. What ſeems odd to me is, that neither M. le Monnier, as appears by his letters to me on this occaſion, nor any of the obſervers at Paris, ſaw Venus ſtretched out as I did, nor any kind of tail: by your deſcription of this tail, there muſt have been ſome little difference in its appearance, to that ſeen by the perſon in my obſervatory here.

If you think it will be acceptable to the Royal Society, I will take an opportunity of forwarding ſome other obſervations; and among the reſt, an account of a very remarkable degree of cold, and its effects on different liquids in the beginning of the year 1768, to which I attended with great attention. In the mean time, I am,

Dear Sir,

Your moſt obedient

humble ſervant,

Nath. Pigott.

XXIV. *Ob-*

Received March 8, 1770.

XXIV.* *Obſervations on the proper Method of calculating the Values of Reverſions depending on Survivorſhips: By* Richard Price, *D. D. F. R. S.*

Read May 10, 1770.

ALL queſtions relating to the values of lives and reverſions, are at preſent of particular importance in this kingdom. Much buſineſs is continually tranſacted in this way; and any conſiderable errors in the method of ſolving ſuch queſtions muſt in time produce very bad conſequences. The deſign of the following obſervations is, to point out a particular error, into which there is danger of falling, in finding the values of ſuch reverſions as depend on ſurvivorſhips. In doing this, I ſhall, in order to be as plain as poſſible, take the following caſe.

" A, aged 40, expects to come to the poſſeſſion " of an eſtate, ſhould he ſurvive B, aged likewiſe " 40. In theſe circumſtances, he offers, in order to " raiſe a preſent ſum, to give ſecurity for 40 *l. per ann.* " out of the eſtate *at his death*, provided he ſhould " get into poſſeſſion; that is, provided he ſhould ſur- " vive B. What is the ſum that ought now to be

" advanced to him in conſideration of ſuch ſecurity, " reckoning compound intereſt at four *per cent.*?"

M. de Moivre's directions, in his treatiſe on annuities, prob. XVII. and XX. lead us to ſeek the required ſum in this caſe, by the following proceſs.

Find firſt, the preſent ſum A ſhould receive for the reverſion of 40 *l. per ann.* for ever after his death, ſuppoſing it not dependent on his ſurviving B.—The preſent value of ſuch a reverſion, is the value of the life ſubtracted from the perpetuity *. The value of the life, taken from Mr. de Moivre's tables, is 13.2 years purchaſe. This ſubtracted from 25, the perpetuity, leaves 11.8, the value of the ſuppoſed eſtate after the life of A; which value therefore, is in money 472 *l.* But (as M. de Moivre obſerves), the lender having a chance to loſe his money, a compenſation ought to be made to him for the riſk he runs, which is founded on the poſſibility that a man of the age of 40 may not ſurvive another perſon of the ſame age. This chance is an equal chance; and therefore *half* the preceding ſum, or 236 *l.* is the ſum which ſhould be advanced now on the expectation mentioned.

This ſolution carries a plauſible appearance; and moſt perſons will, probably, be ready to pronounce it right; nor will this be any wonder, as ſo great a maſter of theſe ſubjects as M. de Moivre, appears to have been miſled by it. Nothing more is neceſſary to prove it to be fallacious, than proceeding in the ſame way to ſolve the following ſimilar queſtion.

" A, aged 40, offers to give ſecurity for 40 *l. per* " *ann.* to be entered upon at his death, provided it

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* By Prob. XXVI. p. 293, of Mr. Simpſon's Select Exerciſes.

" ſhould happen before the death of B, aged likewiſe 40. What ſum ſhould now be advanced to him for ſuch a reverſion, intereſt being reckoned at 4 *per cent.*?"

In ſolving this problem, agreeably to the method juſt deſcribed; we are to find the value of 40 *l. per ann.* to be entered upon *certainly* at the death of A, and then to multiply this value by the chance that A ſhall not ſurvive B, or by $\frac{1}{2}$; and in this way the anſwer comes out the ſame as that already given. Now, it may be eaſily ſeen that this muſt be wrong. The value of a reverſion to be received when a perſon of a given age dies, cannot be the ſame whether the condition of obtaining it is, that he ſhall die *before*, or that he ſhall die *after* another perſon; that is, whether it is provided that a purchaſer, if he ſucceeds, ſhall get into poſſeſſion ſooner or later. The reverſion in the latter caſe muſt, without doubt, be of leſs value than in the former.

The firſt queſtion here propoſed reſolves itſelf into the following general queſtion. " What is the preſent value of a given reverſionary ſum or eſtate, to be received after the failure of two lives, provided one in particular of them ſhould be the longeſt life?"

Now, the preſent value of an eſtate to be enjoyed for ever after the failure of the longeſt of two lives, is the value of the longeſt of the two lives ſubtracted from the perpetuity. The value of the longeſt of two lives is, it is well known, the value of the two joint lives ſubtracted from the ſum of the values of the two ſingle lives. In the preſent caſe, therefore, it is 9.82 (the value of two joint lives of 40 *l.* *) ſubtracted from twice 13.2, (the value of a ſingle life of 40) that is, 16.58 years purchaſe. And this ſubtracted from 25 (the perpetuity) gives 8.42, which multiplied by 40 gives 336.8 *l.* the value of the given eſtate, were it certainly to be enjoyed after the extinction of the longeſt of two lives both 40; that is, whether one or other of them failed laſt. But that A's life in particular ſhould fail laſt rather than B's, is an even chance. The true value of the reverſion, therefore, is $\frac{1}{2}$ the laſt value, or 168.4 *l.*

In like manner. The ſecond queſtion is the ſame with the queſtion. "What is the preſent value of "40 *l. per ann.* for ever, to be entered upon after the "extinction of two joint lives both 40; that is, "whenever *either* of them ſhall fail, provided the "firſt that fails ſhould happen to be A's life in particular?" And the anſwer is found by ſubtracting the preſent value of the two joint lives from the perpetuity, and multiplying the remainder by $\frac{1}{2}$, or the chance that A in particular ſhall die firſt; and this will give the required value, 303 *l.*

In ſhort, it appears in both theſe caſes, that, according to the firſt method of ſolution, we are to ſubtract from the perpetuity the value of one of the ſingle lives, when, in the *former* caſe, the value of the *longeſt* of the two lives, and, in the *latter* caſe, the value of their *joint continuance*, ought in reality to be ſubtracted. I need not ſay what prodigious errors may often ariſe from hence, and how unfit ſuch a method of ſolution is for practice. The Society in Nicholas-Lane, Lombard-ſtreet, for equitable aſſurances on lives and ſurvivorſhips, have in conſtant practice ſuch queſtions as thoſe now ſtated; and, had they happened to have adopted this method of ſolution, they could not have continued long an advantage to the publick.

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* The value of the two joint lives is here given on M. de Moivre's hypotheſis of an equal decrement of life; and it has been calculated by a rule in Mr. Simpſon's treatiſe on the Doctrine of Annuities and Reverſions, p. 16. M. de Moivre's rules, in the ſecond and third problems of his treatiſe on annuities, give the values of joint lives ſo much leſs than the truth, that they ought never to be uſed.

Mr. Simpſon, in p. 322. of his Select Exerciſes, ſpeaks on this ſubject in the following manner. "I "have been very particular on theſe kinds of pro-"blems; and the more ſo, as there has been no "method before publiſhed, that I know of, by which "they can be rightly determined. It is true, the "manner of proceeding by firſt finding the proba-"bility of ſurvivorſhip (which method is uſed in my "former work, and which a celebrated author has "largely inſiſted on in three ſucceſſive editions) may "be applied to good advantage when the given ages "are nearly equal; but then it is certain, that this is "not a genuine way of going to work, and that the "concluſions hence derived are at beſt but near ap-"proximations." This accurate and excellent mathematician has here expreſſed himſelf much too favourably, of the method of ſolution on which I have remarked. In both the caſes I have ſpecified, the ages are equal; and yet in one of them the error is a good deal above a third of the true value, and in the other a fifth: And it is obvious, that in caſes where three equal lives are taken, the errors will be much greater. Mr. Simpſon's obſervations in this paſſage, are true only, when applied to a different method uſed by himſelf in the 28th and following problems of his Treatiſe on the Doctrine of Annuities and Reverſions. This method is exact when the lives are equal; but it gives reſults that are too far from the truth, when there is any conſiderable inequality between the lives.

It is with reluctance I have made ſome of theſe remarks. M. de Moivre has made very important improvements in this branch of ſcience, and the higheſt reſpect is due to his name and authority. This, however, only renders theſe remarks more neceſſary *.

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* The ſtrict demonſtration of the ſolution I have given of the queſtions here ſtated, is as follows. It is plain that the purchaſer of A's right, as ſtated in the firſt queſtion, cannot get into poſſeſſion till the year when A and B ſhall be both dead, nor then, unleſs A happens to die laſt. Now, ſuppoſing the common complement of life n, the probability that A and B ſhall be both dead at the end of the firſt year, and A die laſt, is $\overline{1 - \frac{n-1}{n}} \times \overline{1 - \frac{n-1}{n}} \times \frac{1}{2} = \frac{1}{2} - \frac{n-1}{2n} - \frac{n-1}{2n} + \frac{\overline{n-1}|^2}{2n^2}$. In like manner, the probability that they ſhall be both dead at the end of the ſecond and third, &c. years, and A ſurvive, is $\frac{1}{2} - \frac{n-2}{2n} - \frac{n-2}{2n} + \frac{\overline{n-2}|^2}{2n^2}$, $\frac{1}{2} - \frac{n-3}{2n} - \frac{n-3}{2n} + \frac{\overline{n-3}|^2}{2n^2}$, &c. The preſent value, therefore, of the 1ſt, 2d, and 3d, &c. rents of the reverſionary eſtate, is $\frac{1}{2r} - \frac{n-1}{2nr} - \frac{n-1}{2nr} + \frac{\overline{n-1}|^2}{2nr^2r}$, $\frac{1}{2r^2} - \frac{n-2}{2nr^2} - \frac{n-2}{2nr^2} + \frac{\overline{n-2}|^2}{2n^2r^2}$, $\frac{1}{2r^3} - \frac{n-3}{2nr^3} - \frac{n-3}{2nr^3} + \frac{\overline{n-3}|^2}{2n^2r^3}$, &c. r ſignifying 1 *l.* increaſed by its intereſt for a year; and the eſtate being 1 *l. per annum.* And the ſum of theſe terms is the

In the practice of aſſurances on the ſurvivorſhip of one life beyond another, there is frequent occaſion for making ſuch aſſurances, for a term of years only. The method of determining the value in ſuch caſes I have not ſeen any where deſcribed. Perhaps, therefore, a brief account of it here may be of ſome uſe.

By reaſoning in the ſame manner with Mr. Simpſon, p. 322, Select Exerciſes, it will appear, that the value of any given ſum S aſſured, for *n* years on a given life A, provided in that time another given life B ſhould ſurvive A, is $\frac{S}{a} \times \overline{\frac{b-1}{r} + \frac{b-2}{r^2}} + \frac{b-3}{r^3}$, &c. (continued to *n* terms) $+ \frac{S}{a} \times \frac{1}{2br} + \frac{1}{2br^2} + \frac{1}{2br^3}$, &c. (continued likewiſe to *n* terms) *r* denoting 1 *l.* increaſed by its intereſt for one year; and *a* and *b*, the numbers in the table of obſervations alive at the ages of A and B, divided by the quotient ariſing from dividing the ſum of the differences in the table from theſe

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value required. But $\frac{1}{2r} + \frac{1}{2r^2} + \frac{1}{2r^3}$, &c. is half the perpetuity; and $\frac{n-1}{2nr} + \frac{n-1}{2nr} - \frac{\overline{n-1}|^2}{2n^2r} + \frac{n-2}{2nr^2} + \frac{n-2}{2nr^2} - \frac{\overline{n-2}|^2}{2n^2r^2} + \frac{n-3}{2nr^3} + \frac{n-3}{2nr^3} - \frac{\overline{n-3}|}{2n^2r^3}$, &c. is half the value of the joint lives ſubtracted from half the ſum of the values of the two ſingle lives; that is, half the value of the longeſt of the two lives by problem IV. of M. de Moivre's Treatiſe on Life Annuities. A ſimilar demonſtration may be eaſily applied to the ſolution of the other queſtion.

The beſt rules for finding, in all caſes, the values of reverſions depending on ſurvivorſhips, are Mr. Simpſon's, in his Select Exerciſes, p. 297, &c.

ages reſpectively for n years, by n. Now $\frac{b-1}{br}+\frac{b-2}{br^2}$, &c. (continued to n terms) is the value of an annuity on the given life B for n years. And $\frac{1}{2br}+\frac{1}{2br^2}$, &c. (continued to n terms) is the value of an annuity certain for n years, divided by $2b$.

The general rule, therefore, is this.

" Find (by problem 23. in M. de Moivre's
" Treatiſe on Annuities, fourth edition) the value of
" an annuity on the life of B for n years. To this
" value add the quotient ariſing from dividing by $2b$,
" the value of an annuity certain for n years, taken
" out of M. de Moivre's tables in the treatiſe juſt
" mentioned, or out of table III. in Mr. Simpſon's
" Select Exerciſes: and the ſum multiplied by $\frac{S}{a}$
" will be the required value."

EXAMPLE. Let the rate of intereſt be 3 *per cent.* or $r=1.03$.

The table of obſervations, Mr. Simpſon's in his Select Exerciſes, p. 254.

Let the age of A be ſeven years. B, 30. $n=14$ and $S=100\,l.$

The ſum of the decrements in the table for fourteen years from ſeven years of age, is 73; which divided by 14 is 5.2. The number alive at ſeven is 430; and this, divided by 5.2, gives 82.6 for the value of a. In like manner the value of b may be found to be 41.7. The value of an annuity for 14 years on a life of 30, is 9.5. The value of an annuity certain for 14 years is 11.296, which divided by $2b$ or 83.4, gives 0.13; and this added to 9.5, and the ſum multiplied by $\frac{100}{82.6}$ gives 11.66, or 11 *l.* 13 *s.* for the value in preſent payment of 100 *l.* aſſured to a perſon 30 years of age, and payable to him at the death of a child ſeven years of age, provided that ſhould happen before his own death in 14 years.

It deſerves to be particularly remarked, that in this method likewiſe may be determined, what ſums ought to be paid on any ſurvivorſhip, within a given term of years, of one life beyond another, in conſideration of any given ſums now advanced.

The following example of this is a caſe which has offered itſelf in practice, and which I have had occaſion particularly to conſider.

" A perſon aged 30, having in expectation an " eſtate which is to come to him, provided he ſurvives a minor aged ſeven before he comes of " age, wants in theſe circumſtances to raiſe " 1000 *l.* What reverſion, depending on ſuch a " ſurvivorſhip, is a proper equivalent for this ſum " now advanced, intereſt being reckoned at 3 *per cent.* " and the probabilities of life being ſuppoſed the ſame " with thoſe in the London table of obſervations?"

Anſwer. It appears from what has been juſt determined, that, for 11 *l.* 13 *s.* now advanced, the proper equivalent in theſe circumſtances is 100 *l.* to be paid, in caſe the ſuppoſed ſurvivorſhip ſhould take place. By the rule of proportion, therefore, it will appear, that for 1000 *l.* the proper equivalent is 8576 *l.*

XXV. *De Athmoſphæra Electrica* Joannis Baptiſtæ Beccariæ, *R. S. S. ex Scholis Piis ad Regiam* Londinenſem *Societatem*, *Libellus*.

THEOREMA I.

Corpus B nullatenus electricum, quod mergitur in atmoſphera corporis A *quoquomodo electrici, dum communicat cum ſolo, adipiſcitur electricitatem contrariam electricitati corporis* A.

Read May 17, 1770.

ESTO electrometrum, bacillus nempe ex cera ſignatoria ſeſquipedem longus, cujus extremo uni nectantur ſtamina duo ex lino ſubtiliſſima (vulgo *fiandra*) ut vix æquent unam vigeſimam quintam lineæ partem; longa ſunto duo hæc ſtamina lineas viginti octo circiter, & fruſtulis diſtendantur ex ſubere, aut ex charta quam tenuiſſimis.

Prehendo bacilli caput, & ſtamina admoveo catenæ, ut ipſam attingant; ſi bacillus ſiccitate ſua, & tempeſtate ignem electricum vere coerceat, ſtamina diſcedent ambo in averſam a catena plagam, fient ambo electrica exceſſu.

Imo & ſi catenam non attingant, ſi tamen mergantur ſatis alte in atmoſphæram ipſius, haurient adhuc ejus electricitatem.

Haurient adhuc citius, & plenius, ſi, manente bacilli ſiccitate, humeſcant ſtamina; iterum haurient facilius electricitatem urgentem, quam prementem; dico autem *urgentem*, quæ novo conſtanter ignis electrici motu invaleſcit, aut inſtauratur, ceu cum vitrum pergit fricari. Contra dico *prementem*, quæ, ubi invaluit, non inſtauratur ſimiliter.

Itaque cum in atmoſphæram catenæ præſertim prementem electrometri ſtamina immitto, & tango, arrigunt ſe, tenduntque recta ad catenam, ſive antea penderent, ſive forte etiam diſcederent a catena; aut, ſi erant jam arrecta, diſtenduntur adhuc magis.

Dein, ſi ſtamina ſic tacta, ſemoto digito, relinquam in atmoſphæra, tum digito iterum perſequar, fugiunt a digito in partem averſam, niſi quatenus eam fugam remoratur directio in catenam, qui motus componitur cum vi recedendi a digito.

Si, ubi ſtamina tetigi intra atmoſphæram catenæ, catenam prehendo, dum hujus electricitas perit, ſtamina augent mutuam divergentiam, accedunt jam ad catenam, quæ ſit ſatis proxima, aut ad digitum; ſed admota machina, quæ ſit electrica, ab ipſa diſcedunt; a qua etiam diſcedunt, etſi a catena ſemoveantur, antea quam electricitas catenæ perimatur.

Quare duo ea ſtamina cum tangerentur intra atmoſphæram catenæ electricæ exceſſu, electricitatem nanciſcebantur, quam habet machina, fiebant electrica defectu.

Sed & digitus immiſſus in atmoſphæram catenæ, quamdiu in ipſa merſus manet, debet defectu electricus evadere pariter, ac ſtamina; tum quod par eſt ratio, tum quod paria edit effecta. Quemadmodum enim ea ſtaminum electricitas facit ipſorum divergentiam;

tiam; ita diſceſſio ſtaminum a digito parem in hoc demonſtrat electricitatem.

Si experimentum capiatur intra atmoſphæram machinæ, obtingit ipſum ſimiliter; ſtamina, & digitus fiunt electrica exceſſu, dempta electricitate a machina ad machinam accedunt, aut ad digitum; allata ad catenam ab ipſa recedunt.

Eadem experimenta commodius, & expeditius capiuntur in atmoſphæris phialæ ad exploſionem electricæ; quam dum ſocius unco ſuſtentat, immitto ego electrometrum in atmoſphæram faciei exterioris rite indutæ (ventrem phialæ nominabo) tango ſtamina merſa; ſocius phialam ventre prehendit * ſtamina refugiunt uncum, quem ambit atmoſphæra faciei interioris; tum pergo experiri. Tango ſtamina merſa in atmoſphæra unci; arrigunt ipſa ſe ad uncum; ſocius ventrem phialæ dimittit, uncum prehendit; ſtamina diſcedunt a ventre.

Quare univerſe id conſtat; corpus B, quod intra atmoſphæram corporis A communicat cum ſolo, electricitatem adipiſci contrariam electricitati corporis A; videbimus enim experimenta eadem obvenire in deferentibus aliis corporibus ſimiliter ac in ſtaminibus.

THEOREMA II.

Corpus B *merſum in atmoſphæra corporis* A *exceſſu electrici, dum cum ſolo communicat, fit electricum defectu,*

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* "Id ſemper ago, ut quoties unco experior, phiala ventre "prehendatur, quoties experior ventre, unco ſuſtineatur; quæ "res Franklinianæ theoriæ eſt apprime conſentanea. Interea "cavendum ne phiala eodem tempore unco attrectetur, & "ventre."

quod ignis ipſius disjiciatur in ſolum ab atmoſphæra electrica exceſſu. Et corpus B *merſum in atmoſphæra corporis* A *defectu electrici, dum cum ſolo communicat, fit electricum exceſſu, quod ignis a ſolo in ipſum ſe expandat contra ambientem atmoſphæram electricam defectu.*

Cylindrum cavum ex lamina ferrea ſtamno imbuta clauſum parte ima, ſumma apertum, altum pollices quindecim, latum pollices ſex cum dimidio dico *puteum electricum*; quo quoties experior impoſitum ſuppono ſemper menſulæ rotundæ prælongo vitreo fulcro.

Cylindrum ex ligno ſolido diametro lineas quindecim, altitudine pollices novendecim conveſtitum charta inaurata ex laqueari ſtaminibus ſericis ita ſuſpendo, ut axe ſuo reſpondeat axi putei electrici, & diſtet undique a peripheria, & fundo ipſius pollices quatuor, lineas ſex; *animam putei* dico.

Rebus ita paratis, dextera aſperiorem virgæ metallicæ apicem dirigo in prominentem a puteo animæ partem, læva ventrem phialæ complectens, admoveo uncum ipſius (electrica eſt ad exploſionem) ad latus putei; exſiſtit ſcintilla ab unco ad puteum; ſtellula ab anima putei emicat in apice virgæ; hauſto ab anima igne, ſi quis eſt, reliquo, anima eſt electrica defectu ab atmoſphæra putei electrici exceſſu. Illa repellit ſtamina electrometri, quæ tetigerint ventrem phialæ, quæque attrahuntur a puteo.

Si digito demo exceſſum a puteo, pergit anima eſſe electrica defectu; ſi demo defectum ab anima, pergunt ſtamina eſſe defectu electrica, ſed accedunt jam ad digitum, a quo intra eam atmoſphæram diſcebant; ſtamina enim, utpote ſejuncta a ſolo ope bacilli, manent electrica defectu pereunte atmoſphæra; quam electricitatem continuo amittit digitus, quippe qui cum ſolo communicat.

Itero experimentum; ſed adigo ad puteum ventrem phialæ prehenſæ unco. Emicat ex virga animæ exhibita penicillus; anima eſt electrica exceſſu ab atmoſphæra putei electrici defectu. Anima repellit ſtamina electrometri electrica ab unco phialæ, quæ puteus attrahit, &c.

Quibus experimentis & primum confirmatur theorema, & demonſtratur hoc alterum.

THEOREMA III.

Corpus B *merſum in atmoſphæra electrica corporis* A, *quamquam intra eam non tangatur; tamen adipiſcitur electricitatem aliquam contrariam electricitati corporis* B; *ſed lentius, prout eſt accuratius ſejunctum a ſolo.*

Suſpendo electrometrum, ut bacillus inſiſtat axi putei ſurſum producti, & ſtamina quam proxime immineant ori ipſius; tum unco phialæ ſcientillam immitto in latus putei; ſtamina continuo incipiunt, & pergunt divergere alterum ab altero, & diſcedere ambo a digito eminus admoto.

Porro vidimus in ea atmoſphæra electrica exceſſu digitum continuo evadere electricum defectu; itaque & ipſa, quæ diſcedunt a digito, ſtamina, defectu electrica fieri eſt neceſſe.

Re enim vera ſtamina eadem, quamdiu merſa manent in eadem atmoſphæra, diſcedunt a ventre phialæ electricæ defectu.

Qua

Qua vero ratione fiant defectu electrica, dum coercente ſejunguntur bacillo, id hic pervidendum; atque primo animadverto: raro admodum perfecte coercere electricum ignem ipſa ea corpora, quæ ipſi huic muneri aptiſſima perhibentur. Quod norunt omnes, qui in hoc experimentorum genere verſantur ſatis familiariter.

Addo deinde: ne tum quidem cum optime coercent, omnem omnino intercludere ignis electrici motionem. Funiculi ſerici, quibus ſuſpendo catenam, columellæ vitreæ, quibus fulcio machinam, nudi ſpeculorum Franklinianorum margines, tum etiam, cum optime coercent, aliquam tamen contrahunt electricitatem. Scilicet ea ſola electricitatis pars, quæ a medio deferente vi ſua effundit ſe in limites coercentis, atque in eos progrediens ad intervallum ipſi ſuæ vi proportionale, ordinatim decreſcit, ea, inquam, ſola vim coercendi perficit.

Quare dicendum: ſtamina, quamquam bacillo ſejuncta, poſſe tamen ignem dimittere aliquem ſuum vi atmoſphæræ exceſſu electricæ pulſum in partes aeris remotioris, aut in partes bacilli ipſius, ad quas aut atmoſphæra omnino non pertingit, aut pertingit minus denſa, atque ea ratione electricitatem aliquam tenuem invaleſcere in ſtaminibus.

Tenuem, inquam, nam ſtamina, quum primo moventur ab admoto digito, recedunt; ſed, ubi digitus accedit paullo propius, ipſum appetunt. Qui eſt genius eorum corporum, quæ electricitatem habent quidem homologam, ſed alterum habet minimam, alterum ſatis magnam.

Re enim vera ſtamina ea electricitate, quam contrahunt ſola immerſione, divergunt minus, quam ea, quæ invaleſcit, dum tanguntur, illaque durat longe minus hac. Si manu puteum tango, & digito, continuo adſum ſtaminibus, affluunt ipſa adhuc ad digitum; ſi digitum admoveo ſtaminibus paullo tardius, hæc jam hærent in nativa directione electricitate ea tenui prorſus deſtituta, quam ſervant validam extra ipſam electricam atmoſphæram, cum ab attrectatione contraxerunt.

THEOREMA IV.

Atmoſphæra electrica poteſt electricitatem homologam debiliorem demere a corpore merſo; & indere contrariam.

Experimentum capio alternatim unco, & ventre phialæ tangendo latus putei. Quod experimentum quoniam plurimas habere poteſt, & pro atmoſphæræ varietate, & pro tentamine vario, differentias, ipſum primo tribuo in partes ſuas, notaturus deinde diverſos partis cujuſque modos.

Itaque I. Dum unco tango latus putei, ſtamina imminentis electrometri divergunt. II. Cum dein puteum tango ventre phialæ, ſtamina conveniunt. III. Mox iterum divergunt. IV. Si iterum tango unco, ſtamina ſimiliter conveniunt, iterum mutuo diſceſſura.

Atque de prima quidem experimenti parte dictum eſt ſatis in theoremate proximo; quomodo diſcedant ambo ſtamina alterum ab altero, atque ambo a digito, & a phiala; quo limite concludantur eæ diſceſſiones; qua efficiantur cauſa. Atque eadem, quæ de his phænomenis explicuimus, quæ obtinent, cum ſtamina primo divergunt, de ſimilibus ſunt intelligenda, quæ innovantur, cum iterum divergunt, poſtea quam jam ſemel, aut iterum convenerunt.

Quare id unum hic eſt reliquum, ut de modis dicam, queis ſtamina conveniunt, cum mutantur immiſſæ antea in puteum electricitates. Qua in re variæ omnino pro variis adjunctis oboriuntur differentiæ.

Nam, ſi ſtamina validiorem hauſerint defectus electricitates, dum primo in athmoſphæra mergebantur unco excita, cum poſtmodum electricitas defectus ventre phialæ repente immittitur in puteum, tum ſtamina defectum ſervantia ſuum, ceu corpora ſimiliter electrica, reſpuuntur a puteo, ſurſum efferuntur in adverſam ab ore ipſius plagam, erectaque ſuſpenduntur, & ſuſpenduntur tanto diutius, quanto magis cohibens eſt bacillus.

Quandoque (id autem præſertim fit, cum ſtamina ad aliquam in os putei merguntur altitudinem) ſtamen alterum arrigit ſe, ſurſum manente altero depreſſo, quaſi accommodent ſe igni electrico aptiſſime tranſvehendo, qui denſior eſt in locis puteo propioribus, rarior in remotioribus.

Quum ſtamina aut ambo, aut eorum alterum, quod unum ſurſum rejectum ſit, deſcenderunt deorſum, pergunt divergere, nec conveniunt, niſi lente admodum, & ventre phialæ urgendo ſemper latus putei, quo manens validior defectus putei defectum denique perimat ſtaminum debiliorem.

Atque cum ita ſe habet experimentum, tum ſtamina ſive ſint adhuc erecta, ſive jam deſcenderint, ſemper, antea quam conveniant, accedunt ad digitum; nam manent electrica defectu, interea dum digitus in atmoſphæra electrica defectu eſt exceſſu electricus.

Pro adjunctis contrariis, contraria ſunt effecta; videlicet, ſi ſtaminibus debilem atmoſphæra unco excita intulerit electricitatem defectus, neque illatam bacillus ſatis apte coerceat, tum, admoto ventre phialæ, ſtamina continuo conveniunt, atque iterum divergunt. Imo plerumque, præſertim in ore putei, ne conveniunt quidem, ſed vix aliquantulum, & ad momentum temporis minuto divergentiæ angulo, ipſum iterum augent; ſcilicet momento temporis perit atmoſphæra exceſſus, & invaleſcit atmoſphæra defectus, quæ debilem defectus electricitatem perimit in ſtaminibus ſatis alacriter, immittitque continuo aliquam exceſſus electricitatem.

Sed & his adjunctis commode ſatis experior theorematis veritatem: nam tum filum ex lino necto lateri putei longum ſeſquipollicem, atque huic admovendo phialæ ventrem, id aſſequor, ut lente ſatis, ac pedetentim pereat electricitas exceſſus in puteo, & defectus in ſtaminibus, mox lente electricitas defectus in puteo, & exceſſus in ſtaminibus invaleſcat. Quorum primum dum fit, pergunt fila diſcedere a digito, ſed uſque, & uſque minus. Dum enim minuitur uſque, & uſque magis electricitas exceſſus in puteo, minuitur electricitas defectus in digito, & in ſtaminibus, pro quo decremento diſceſſionem ſtaminum a digito decreſcere oportet.

Imo id plerumque accidit, ut, antea quam ſtamina prorſus conveniant, convolent ad digitum; etenim electricitatem atmoſphæræ putei, neceſſe eſt perire omnem, anteaquam omnem, quam ex ea hauſerant ſtamina defectus electricitatem, prorſus amiſerint.

Poſtea quam ſtamina convenere ad nativam gravium directionem, tum ex invaleſcente electricitate defectus in puteo oboritur exceſſus in ſtaminibus; ipſa iterum divergunt, quemadmodum principio divergebant ab electricitate defectus ſua, quæ exſiſtebat ex exceſſu in puteum immiſſo.

Quod ſi iterum unco, mox iterum ventre phialæ alternatim urgeatur puteus, alternatim ſtamina accedunt, diſcedunt, atque ita oſcillantur, quemadmodum explicui, prout immiſſæ antea electricitates pereunt, invaleſcuntque contrariæ.

THEOREMA V.

I. *Corpora ſatis alte merſa intra cavitatem ſatis arctam corporis deferentis, quamdiu ab omni exteriore corpore ſunt accurate ſejuncta, electricitatem nullam contrahunt ab electricitate, quæ immittitur in corpus deferens cavum, nec quæ tangunt faciem cavitatis, nec quæ intra ipſum ſuſpenduntur diſſita ab ipſius facie.* II. *Quæ ſuſpenduntur intra cavitatem diſſita ab ipſius facie, quam primum communicant cum corpore exteriore, adipiſcuntur electricitatem contrariam electricitati immiſſæ in corpus cavum, & proportionalem capacitati corporis exterioris, quo cum communicant:* III. *Atque tum ſolum æqualis pars electricitatis immiſſæ in corpus cavum, explicat ſe in faciem interiorem cavitatis, & in corpora, quæ ipſam tangunt, ceu in partes ejus faciei.*

Cavitas putei noſtri ſatis arcta eſt pro hoc inſtituto, præſertim ſi phiala ſit valide electrica, quo validam in ipſo excitet electricitatem; atque corpora in ejus cavo ſatis alte merſa erunt, ſi deprimantur infra dimidiam altitudinem ipſius.

Primo itaque imo lateri cavitatis necto tenuiſſimum ſtamen, ut juxta ipſum pendeat; chartulam hoc habet annexam, quo cerni poſſit.

Tum electrometrum demitto, cavens antea, ut ſicciſſimum ſit; atque ideo tempeſtate experior ſicciſſima, prætereaque bacillum electrometri igni admoveo.

Rebus ita comparatis, puteo ipſi etiam accuratiſſime ſeparato ſcintillam immitto unco; mox aliam ventre phialæ elicio validiorem, aliam unco immitto, ventre elicio aliam; atque ita deinceps.

Quæ dum fiunt, introſpicio diligenter; neque interea nutant quidquam aut ſtamina electrometri, aut ſtamen aliud puteo nexum. Imo hæc lateri putei admota ipſum non appetunt. Videlicet nulla in ſtaminibus electrometri, nulla in ſtamine putei, nulla in ima ipſius facie interiore cernitur electricitas.

Itero experimentum electricitates in puteum immittens lente, & pedetentim; uncum nempe, mox ventrem phialæ admovendo perbrevi lineo ſtamini, quod exterius eſt puteo nexum; ſimiliterque tranquilla manent omnia: nulla in ſtaminibus electrometri motio, nulla in ſtamine putei, nulla ſtaminum electrometri ad puteum propenſio. Porro, quando ſtamina electrometri ſuſtinebantur in ore putei, quod aliquam ibi contraherent electricitatem a putei electricitate, divergebant, & a mutata in contrarium electricitate putei, conveniebant, atque iterum divergebant.

Itaque, quoniam nunc ſuſpenſa ſtamina in ima putei cavitate nullatenus commoventur, nec ab immiſſa, nec a dempta electricitate, neque a mutata in contrarium;

rium ; liquet electricitatem ipſa contrahere omnino nullam.

Re enim vera poſt immiſſam, & mutatam utlibet putei electricitatem, demo omnem ; ſtamina continuo extraho, digitum nullatenus appetunt meum, niſi forte electricitatem inde contraxerint aliquam, quod bacillus, aut aer non eſſent ſicciſſimi; quod cum ſit ſtamina electricitatem reſpondentem oſtendunt, ſed contrariam putei electricitati.

Adde, demerſa electrometri ſtamina nedum electricitatem acquirere ullam ab electricitate putei ; ſed ne mutare quidem ullatenus, ſi quam habebant antea. Unco, aut ventre phialæ tango ſtamina electrometri ; tum divergentia ſuſpendo in ima, & media putei cavitate, puteum exterius tango ventre, tum unco, iterum ventre, iterum unco, &c. ſtamina electrometri non nutant, tenent divergentiam ſuam. Demo a puteo inditam, ſi qua eſt reſidua, electricitatem ; ſtamina extracta pergunt divergere ſimiliter. Si principio tacta ſunt unco, uncum ; ſi ventre phialæ, ventrem pergunt refugere.

Scilicet electricitas ſtaminibus electrometri principio illata, nullatenus mutatur ab illata, ne contraria quidem, in puteum electricitate *.

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* " Quæ quum primo ſum expertus excurrebat mihi animus " ad propoſitionem Princ. Newt. num. ſeptuageſimam : ſi " ad ſphæricæ ſuperficiei puncta ſingula tendant vires æquales " centripetræ decreſcentes in duplicata ratione diſtantiarum a " punctis, dico, quod corpuſculum intra ſuperficiem conſtitutum " his viribus nullam in partem attrahitur ; porro in infiniti cy- " lindri cavitate, quemadmodum in ſphæra cava ſimiles ſunt quæ " ad verticem opponuntur evaneſcentes pyramides ; ſimiles etiam " in cylindro finito, quæ pertingunt ad oppoſita cylindri latera. " Quo ex principio demonſtratio conficitur pro ſphæra cava. Sed " de his alias."

Sed ne in interiorem quidem, & imam putei faciem patet, quæ in ipſum immittitur, electricitas: cilindrum cavum, clauſum undique, rotundata baſium perimetro altum pollices duos, tres latum, ſuſpenſum ſtaminibus ſericis tribus (ſitulam dico), dimitto in puteum (in quem aut unco, aut ventre phialæ, aut antea immiſi, aut immitto poſtmodum electricitatem), ut tangat fundum ipſius, aut non tangat; tum recta extraho juxta axem ipſius; exploro digito, manu inverſa, aut etiam gena; ſcintilla nulla; admoveo alias electrometri fila; vix commoventur, & plerumque omnino non commoventur. Interea tamen, ſi ſitulam hanc ineptiſſimam electricitati haurendæ ex interiore putei capacitate puteo admoveo exterius, fit electrica primo, ſcecundo, tertio, ut ſemota ſcintillam aut acceptam a puteo reſtituat omnino ſenſilem, aut immiſſam in puteum recipiat *.

Sed faciamus jam aptam electricitati hauriendæ ineptam hactenus ſitulam; demitto ipſam rite ſtaminibus ſericis ſuſpenſam in imum puteum, ſed ne puteum ipſa tangat. Tango ego ipſam virga metallica in os putei (electricitate hic imbutus ſupponitur) immiſſa; ſcintillam virgam inter, & ſitulam emicantem cerno, audio; rem elata ſitula digitum pungit mihi.

Experimentum itero; ſitulæ tactæ intra imum puteum mox elatæ admoveo ſtamina electrometri electrici ab unco phialæ; ſi puteus electricus erat ab unco, ſtamina electrometri ſitulam appetunt, quo refugiant,

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* " Horum, quæ profero experimentorum, hoc unum meum " non eſt; ſed narratur a D. de Sauſſure in theſibus, quas " Genevæ edidit, anno 1767, *de re electrica.* Qui ſibi narratum teſtatur a civibus Genevenſibus, ipſiſque narratum Londini " a Franklinio."

electrica hæc ſint oportet a ventre phialæ; et viciſſim, ſi puteus erat electricus a ventre, ſtamina electrometri, ut refugiant ſitulam, ab unco debent eſſe electrica.

Quare ſitula ſuſpenſa in ima cavitate putei tum ſit electrica, cum communicat cum corpore exteriore; ſed electricitatem tum adipiſcitur contrariam electricitati immiſſæ in puteum.

Atque hoc in experimento ſitula electricitatem contrahebat vividam, quod communicaret cum ſolo; e ſolo enim metallica virga ipſam ego attingebam.

Repeto experimentum: attingens ego ipſam ſcamno inſiſtens cohibenti; ſit electrica, ſed multo minus.

Repeto; ſed virgam longam pollices octodecim bacillo teneo ex cera ſignatoria adfixo ſummo capiti ipſius; ſcintillam non cerno, non audio. Stamina electrometri prædita electricitate contraria electricitati putei refugiunt quidem a ſitula; ſed refugiunt parum admodum pro modica ſitulæ electricitate, quæ nempe modicæ reſpondet virgæ metallicæ capacitati.

Ea oſtenduntur omnia ſtaminibus electrometri demerſis, quæ cum virga metallica attingo, continuo divergunt, & intra cavitatem putei refugiunt deinde a virga, quæ nempe parte ſua imâ quemadmodum ſtamina electrometri, electricitatem contrahit contrariam electricitati putei.

Si ſtamina electrometri demittantur in puteum electrica, adeoque divergentia, tum puteo indatur electricitas; ſi hæc ſit homologa electricitati ſtaminum, ipſa immiſſam virgam appetunt, conveniunt; iterum tacta virgam refugiunt. Sin electricitas putei ſit contraria electricitati ſtaminum, ſtamina etiam principio immiſſam virgam refugiunt; ſed ubi ipſa aſſequor, divergunt adhuc magis, & virgam refugiunt longius.

Sed id nunc oſtendendum, dum contraria electricitas communione cum corpore exteriore excitatur in corpore ſuſpenſo intra imam cavitatem putei, interea proferre ſe e perimetro interiore & imo putei, & in corpora, quæ ipſum tangant, portionem electricitatis in puteum immiſſæ.

Virgam metallicam bacillo tenens ex cera ſignatoria admoveo ſtamini, quod pendet juxta imum putei latus; hoc illam appetit; appetit alacrius, ſi virgam ipſam manu tango, quod electricitas contraria invaleſcat vividior in parte virgæ demerſa, & vividior pariter propria putei electricitas prodeat in nexum ſtamen.

Sed in hoc experimento aliqua manet ſuſpicio, ne ſtamen electricitate moveatur ſola, quæ gignitur contraria in merſa virgæ parte; agedum experimento rem conficiamus. Situlæ hactenus adhibitæ ſtaminibus ſericis tribus ſubnecto ſitulam aliam æqualem, & ſimilem ut inferior hæc diſtet ab ea ſeſquipollicem circiter. Tum demitto ambas in imum puteum, ut inferior fundum attingat, ſuperior ſuam teneat ab inferiore ſeſquipollicis diſtantiam. Immitto, demo, inverto electricitates in puteum immiſſas, tum ſitulas extraho juxta axem putei, quin communicarint cum ullo corpore exteriore; tentatæ quomodolibet, nullam præ ſe ferunt ſatis ſenſilem electricitatem.

Repeto experimentum; ſed ubi ſitula inferior jam fundum tangit, immiſſa metallica virga tango ſuperiorem; proſilit ſcintilla; dempta virga, elevo iterum ſitulas; extractæ ſunt ambæ electricæ; ſed inferior electricitatem habet homologam electricitati in puteum immiſſæ, & debilem admodum; ſuperior contrariam, atque validam.

Nempe e facie interiore, & ima putei prodit electricitatis propriæ pars æqualis electricitati contrariæ, quæ ob immiſſam virgam excitari poteſt in ſitula ſuperiore; ſed hæc in ſitula ſuperiore contraria electricitas eſt valida admodum, quia ſola reſpondet, & æquatur electricitati toti, quæ prodit e tota interiore putei facie; electricitas illa in ſitula inferiore eſt longe debilior, quia ſitulæ inferiori contingit ſola portio electricitatis propriæ putei, quæ proferebat ſe ex tota interiore, & ima putei facie, cujus faciei modicam partem conſtituebat ſitula inferior *.

Portionem autem electricitatis propriæ putei reſpondentem toti contrariæ electricitati, cui virga metallica facimus aditum, ut ineat in ſitulam ſuperiorem, portionem, inquam, illam explicare ſe ex tota interiore ima putei cavitate, vel inde liquet, quod, quo tempore attingitur ſitula ſuperior, eodem ſtamen nexum interiori, & imo putei lateri diſcedit a latere ipſo.

COROLLARIUM I.

Porro pro re hac non alia inter faciem interiorem, & exteriorem putei intercedit differentia, niſi quod ſpatium illa comprehenſum concludatur undique ab atmoſphæra electrica ambiente puteum exterius, &

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* "Atque hæc contrariarium in vicinis ſitulis electricitatum "differentia facit, ut cautione opus ſit aliqua pro iis explorandis; ſuperior enim longe attrahit ſtamina electrometri, "queis immiſſa ſit electricitas homologa immiſſa in puteum; "quæ ut inferior ſitula repellat, admovenda ſunt imæ ipſius "baſi; ſi vis, ut ſitula inferior repellat undique ſtamina homologæ electricitatis, demenda eſt primum electricitas a ſitula "ſuperiore."

ſuperius; contra in ſpatium ambiens faciem putei exteriorem patet quidem undique electrica atmoſphæra, ſed quæ finitur in aërem nullatenus electricum. Itaque hinc repetenda eſt cauſa, quamobrem corpora merſa in interiore, & ima putei cavitate nullam imbuant electricitatem, quam imbuunt corpora merſa in facie exteriore aut in ore putei. Re enim vera puteum confeci ex charta inaurata deſcripto æqualem, & ſimilem, ſed qui ſtaminibus ſericis explicatur. Quod cum fit, electricitas hiſcit æqualis ex utraque ipſius facie, & æqualiter contrariam electricitatem affectant corpora, quæ in atmoſphæra merguntur faciei utriuſque.

COROLLARIUM II.

Scilicet ignis redundans patere poteſt in atmoſphæram electricam circa exteriorem putei faciem, poteſt ingeri in faciem aëris attigui, quod vibrare poſſit nativum ignem ex conſequentibus aëris ſtratis in ſtrata uſque, & uſque conſequentia, aut etiam in ſolum, aut in merſa quævis corpora deferentia. Contra ejuſdem ignis redundantis pars nulla patere poteſt in aërem attiguum interiori putei cavitati, quod is aër urgeatur undique æqualiter abs igne redundante exterius ſuperius conſtituente electricam atmoſphæram. Unde, quando ignis nativus disjici nullo modo poteſt ex eo aëre interiore, ne ullus quidem redundans ignis poteſt addi ſtrato aëris attiguo interiori faciei.

COROLLARIUM III.

Ratio electricitatis, quæ defectus dicitur, videtur eadem: abs aëre attiguo faciei exteriori putei ignis aliquis nativus ſubduci poteſt, quod in ſtrata conſequentia confluere poſſit ignis nativus (quoad aëris ad ignem electricum habitudo patitur) a ſtratis uſque, & uſque remotioribus, aut etiam a ſolo, aut a corporibus deferentibus. Contra a facie aëris attigui faciei interiori putei ignis nativi ſubduci nulla pars poteſt, quod is aër ab atmoſphæra ſuperius, & undique exterius puteum ambiente ſollicitetur æqualiter, ut ignem nativum deponat ſuum. Unde, quando ignis redundans affluere nullo modo poteſt ad aëris partes remotiores, ne ulla quidem nativi pars demi poteſt ex aëre attiguo.

COROLLARIUM IV.

Cui explicationi ne quid ſubſit minus certum, eam non aliter intelligendam volo, quam quod experientia poſtulat: qua conſtat. I. Ignem redundantem explicantem ſe circa faciem exteriorem corporum, id eniti, & plane efficere, ut ignem nativum disjiciat a corporibus merſis in ſtratis conſequentibus. II. Ignem nativum demptum abs aëre attiguo faciei exteriori corporum id præſtare, ut ignis redundans confluat in corpora merſa in ſtratis conſequentibus. III. Ignem redundantem explicare ſe nullum in aërem attiguum interiori, & imæ putei faciei, niſi quatenus per communionem cum corporibus exterioribus tantumdem ignis poſſit disjicere a corporibus merſis in eo interiore aere.

aëre. IV. Ignem nativum demi nullum abs aëre attiguo interiori, & imæ putei faciei, niſi quatenus tantumdem ignis redundantis poſſit confluere in corpora merſa in eo interiore, ſed remotiore aëre.

COROLLARIUM V.

Jamvero nonne pori corporum ſunt veluti totidem minimulæ evaneſcentes noſtri putei cavitates, quæ aptæ ſint quidem igni electrico tranſvehendo, cum patet ipſi via ad alia corpora (aut potius ad aërem corpora alia ambientem), ceu cum ex ima cavitate putei ſcintilla proſilit ad virgam, aut ad digitum immiſſum; ſed nullum in ſe admittant ignem redundantem, nullam deponant nativi ignis partem, & exceſſum totum, aut defectum contineant in facie exteriore corporis?

COROLLARIUM VI.

In hac hypotheſi intelligitur, quomodo corporum electricitas ſuperficiebus corporum, non maſſis reſpondeat: quam Franklinii opinionem anno 1753, confirmabam; ipſaque hæc res hypotheſim illam plane videtur expoſtulare.

COROLLARIUM VII.

Præterea, ſi pori corporum ullam imbuerent electricitatem, cum ſitulam demitto in fundum putei, tum maxime imbuerent aliquam; ſeu electricitas effuſa in poris putei, & illa etiam, quæ atmoſphæram exteriorem conſtituit, effunderit ſe ad æqualitatem in poros ſitulæ; eaque in ſitula extracta pateret; quam tamen a puteo ſatis alto arctoque, ſitula accurate ſeparata (*inſolata*), haurit nullum.

COROLLARIUM VIII.

Adde: corpus deferens parte una ſua electricitatem habere poſſe aliquam, dum alia habeat nullam, aut etiam contrariam. Ceu evenit cum e ſolo manum ego unam immitto in atmoſphæram catenæ, atque aliam in atmoſphæram machinæ ingero. Similiter cum e ſcamno electrico manum unam immitto in fundum putei, illa, & brachio, qua parte mergitur in atmoſphæra putei, ſum electricus defectu, parte reliqua corporis ſum electricus exceſſu; ſcilicet in aërem puteum interius ambientem expandere ſe poteſt exceſſus puteo immiſſus, quia ex aëre remotiore manum ambiente disjicere poteſt tantumdem ignis in aërem ambientem corpus reliquum extra puteum conſtitutum; atque hæ contrariæ electricitates, quemadmodum experiendo pervidi, ſunt inverſe (quod res plane poſtulat) uti partes, ad quas pertinent. Nempe electricitas defectus in manu tanto vividior eſt electricitate exceſſus in corpore reliquo, quanto latius patere debet circa corpus reliquum ignis, qui in hujus ambitum disjicitur abs ambitu manus. Similiter cum ſitulam demiſſam ad imam putei cavitatem attingo virga metallica, ſitula extracta modicum habet defectum (puteus hic ponitur ſemper electricus exceſſu) virga, quam teneo bacillo cohibente, modicum habet exceſſum.

COROLLARIUM IX.

Eamdem hypotheſim poſtulare videntur experimenta Cantoni adjecta experimentis Franklinii. Eamdem, pulcherrimum Franklinii experimentum de atmoſphæra explicante ſe circa catenam, quæ evolvitur e ſypho vitreo. Eamdem, experimenta illa mea, quæ ſcribebam in ſeptima ad Beccarium epiſtola de electricitate, quam appellabam aëream, quod ipſam effunderem late in aërem, vaporoſum, cujuſmodi aër eſt, qui in clauſis conclaviis halitus imbuit a camino, a fornacula, aut eos etiam, qui expirant plurimi a corpore humano. Porro aſſequor quamfacillime electricitatem hanc aëream fumum excitando ex thure in medio conclavio. Tum urgendo electricitatem catenæ, obſervo; I. ſtamina electrometri, quandiu bacillo ſicciſſimo ſejuncta manent a corpore omni, vix divergere; II. accedere ad digitum admotum defectu electricum; III. Attrectata, divergere mutuo, & diſcedere & a digito, & a ventre phialæ.

COROLLARIUM X.

In hac hypotheſi facere electricum exceſſu corpus deferens idem erit ac immittere ignem electricum nativo copioſiorem in aërem attiguum, ſed ut is vibret ignem nativum ex ſtratis conſequentibus. Facere electricum defectu idem erit ac demere ignem nativum ex aëre attiguo; ſed ut ignis redundans confluat in ſtrata conſequentia.

COROLLARIUM XI.

Itaque ipſa aërea electricitatis affectio pertinet jam ad affectionem, quam in vitro invenit Franklinius, quamque in talco, in ſulphure, in pice, in coloſonio, in reſinis, in corporibus nempe omnibus ſatis apte cohibentibus demonſtravi conſtare eamdem. Scilicet, quemadmodum corpora cohibentia alia, ita & aër corpus fluidum ipſe quidem, ſed & cohibens, quoad ſua patitur fluxilitas, dum admittit ignem redundantem in facie ſua attigua corpori, quod dicitur exceſſu electricum, nativum vibrat e ſtratis conſequentibus; & dum exuit nativum ex facie ſua attigua corpori defectu electrico, redundantem allicit ad ſtrata conſequentia; imo (quod analogiam confirmat) aër non admittit redundantem, niſi nativum vibret expoſito ordine, nec exuit nativum, niſi alliciat redundantem.

COROLLARIUM XII.

Hinc ſcintilla omnis genus eſſe videtur imperfectæ exploſionis; cum digito ſcintillam cieo ex catena, ignis redundans ex facie aëris attigui catenæ init in digitum; intereaque ignis nativus, qui vibrabatur ab eo igne redundante in ſtrata conſequentia, refluit in ſua loca per minimula, ad quæ vibrabatur intervalla; cum ſcintillam digito immitto in machinam, ignis nativus affluit ad attiguum aërem, atque ignis, qui affluxerat per minimula intervalla in ſtrata conſequentia refluit ad loca ſua.

COROLLARIUM XIII.

Certe, cum digitum mergo in atmoſphæra catenæ, jam ignis abs aëre digitum ambiente vibratur in digitum, antea quam ignis redundans in aëre attiguo catenæ appellat ad digitum. Cum mergo in atmoſphæra machinæ, ignis nativus digiti effundit ſe in aërem digito ipſi proximum, anteaquam pertingat ad machinam; quæ etiam res impedit, ne directio ſcintillæ cerni poſſit.

COROLLARIUM XIV.

Univerſe vi electricitatis B non accedit ad A, niſi quatenus A & B aut habent electricitates contrarias, aut alterum alteri immittit contrariam ſuæ. Sic bracteolæ impoſitæ ſicciſſimis ex vitro ex reſina ſtratis non advolant ad catenam, quam appetunt quamprimum digitus ſupponitur iis ſtratis contra bracteolarum loca. Tum enim bracteolæ deponunt nativum ignem in attiguam vitri faciem, quando oppoſita poteſt immittere ſuum in digitum; quod experiendo cognovi: nam bracteola vix diſcedebat a vitro, diſtracta ſtamine ſerico, ne catenam aſſequeretur, inveniebatur defectu electrica.

COROLLARIUM XV.

Id ipſum vidi in tæniis. Tænia ſerica B nullatenus electrica, ſed ſicciſſima ab igne pendet juxta nativam directionem prope tæniam ſociam vehementer electricam; neque hæc illam appetit ullatenus; ſed cum B poteſt dimittere ignem ſuum (ſi A ſit exceſſu electrica) in proximum deferens corpus, tum demum accedit ad A, ipſique adhæret. Qua in coitione redundans in tænia A ignis fundit ſe in tæniam B pro contraria hujus electricitate; ſcilicet contrariæ tæniarum electricitates pereunt quatenus æquales; ſed permanet ad tempus aliquod diſpoſitio frictione, aut aliter immiſſa, qua, dum tæniæ disjunguntur, A & B vindicant ſibi mutuo illa exceſſum, hæc defectum ſuum priſtinum. Quam diſpoſitionem nominabam electricitatem vindicem in epiſtola ad Franklinium eo nomine inſignitam.

P. S. A data illa ad Franklinium epiſtola pervidi poſtmodum electricitatem vindicem in vitris incipere etiam ante exploſionem. Eſto exemplum: duo vitra junctim electrica, cum principio disjunguntur, anteaquam tangantur ullatenus, amittunt partes aliquas electricitatum, quas habent in faciebus, quæ disjunguntur, ſeu quæ mutuo denudantur. Quare vitrum quodque in facie utraque electricitatem demonſtrat congruentem externæ faciei ſuæ. Atque ita ſemel, bis & ter accidit; ſed electricitatis partes, quas amittunt vicibus conſequentibus, ſunt uſque, & uſque minores. Plerumque, cum quarta, aut etiam tertia disjunguntur vice, electricitates ex disjunctione jam demonſtrant nullas in faciebus oppoſitis; ſed deinceps incipiunt oſtendere jam aliquas, & contrarias iis, quas principio demonſtrabant, quamvis retineant adhuc vim explodendi, nec ſint attrectata ſimul in oppoſitis faciebus. Scilicet vitrum quodque incipit jam electricitatem oſtendere congruentem internæ ſuæ faciei, quæ disjunctione denudatur. Atque ita pergunt vitra poſt exploſionem quam diutiſſime vindicare ſibi electricitates eaſdem ex disjunctione, conjunctione & attrectatione amiſſas.

Ad earum vero electricitatum mutationem quod attinet, quæ fit ex inverſione vitrorum, res mihi certo conſtat, quoties invertuntur vitra ambo ſimul; ſed, cum invertitur ſolum vitrum tenuius, inverſio electricitatum quandoque non obtinuit; quod qui fiat perſequar brevi, fuſam, quam vindex electricitas poſtulat, proferendo tractationem.

Taurini, 26 Februarii, 1769.

XXVI. *Four Letters from Mr.* T. S. Kuckhan, *to the Preſident and Members of the Royal Society, on the Preſervation of dead Birds.*

Sir and Gentlemen,

Read May 24, 1770.

CONSIDERING the number of vertuoſi, who apply themſelves to the collecting natural curioſities of the inſect, bird, and beaſt kinds, it is ſurprizing that ſo few have endeavoured to diſcover effectual means of preſerving their curioſities, when collected; one would imagine that thoſe Gentlemen, in particular, who write on natural hiſtory, would be exceedingly deſirous of ſuch effectual methods, which, if once diſcovered and communicated to the public, would be the means of their receiving many rare ſubjects, and even non-deſcripts from different parts of Aſia, Africa, and America, which would afford infinite pleaſure to naturaliſts, and greatly encourage the ſtudy of natural hiſtory. But inſtead of this, it is too common to ſee people, for want of knowing better methods, perſevering in thoſe which their own experience, and that of their acquaintance, daily convinces them are ineffectual: although they have the mortification of ſeeing their collections, which have been made with great trouble and expence, continually dropping into decay. I think I have tryed moſt, if not all, the methods that have been publiſhed or practiſed for many years paſt, with all the care and attention I could, and it was not till after the loſs of much time and many fine ſubjects, birds in particular, that I ſet myſelf to find out ſuch methods, drugs, and liquors, as would effectually penetrate and perfectly cure all the parts, ſo as to keep them plump and full.

Before I proceed to deſcribe my method of preſervation, I ſhall beg leave to ſet down the objections I have to the preſent ways and the materials made uſe of in them: and firſt, with regard to that in which raw allum, common ſalt, and black pepper, are applied, I never could find thoſe materials ſufficient for a perfect preſervation. They never fail to become humid in moiſt air and long continued wet weather; ſuffer the fleſh to rot, and even corrode the wires made uſe of to confine the birds in their natural attitudes, till the whole drops to pieces on the leaſt touch or motion. Salt naturally degenerates to a pickle; if the bird has been killed by ſhot, it will ouſe through the ſhot holes. If it has been killed by hand, an inciſion muſt be made, in order to extract the intrails and put in the materials that are to effect the preſervation. Now it is impoſſible to cloſe that inciſion ſo tight, as to confine the pickle from creeping out, and whenever it does get out, it will infallibly ſpoil the plumage; or if, to prevent that, we hang up the birds by the feet, then the pickle will deſcend to the neck and head, before the upper parts in that ſituation are ſufficiently cured; the certain conſequence of which (in ſummer here, and at all ſeaſons in hot climates) will be, that maggots will be generated in ſuch uncured parts, and of courſe the birds deſtroyed. Suppoſing however for a moment (what will ſcarce be found to happen once in a thouſand tryals) that the pickle ſhould penetrate and cure every part, we have then, What ?—a bird preſerved in its natural ſhape, dimenſions, attitude, and colours. No, but we have a poor ſhriveled-up dried carcaſe of a bird, in which neither the natural ſhape, dimenſions, or colours, are preſerved, and which continullay excites the diſagreeable idea of the poor thing's having been ſtarved to death on purpoſe. It is true the eyes look lively and in full preſervation; and no wonder, for they are glaſs: they ſerve, however, by the contraſt, to ſhew more ſtrikingly the miſerable condition of the reſt of the body. One would have imagined that ſo palpable an abſurdity, as the placing a fine full gliſtening eye in the head of a body, not only manifeſtly dead, but appearing to have periſhed by ſickneſs or famine, would have been obvious to every body; to have kindly ſuffered the languid eyelids to cloſe, would have at leaſt avoided ſo ridiculous a contradiction. Laſtly, experience ſhews that birds thus treated are ſeldom or never ſo cured, but that the fleſh grows rank; that rankneſs invites the inſects, and of courſe the bird is ſoon deſtroyed. A ſecond method of preſerving birds is, by immerging them in ſpirits, and if the barely keeping the carcaſe of birds from putrefaction is all that is required, I muſt own this method is an effectual one, and congratulate the naturaliſts on the facility with which they may now procure foreign birds. We have nothing to do but ſend by ſailors bound to different parts of the world, a few kegs of ſpirits, and we ſhall be ſure of birds enough preſerved in this manner; but then what becomes of their proportions, attitudes, graces, and in ſhort, of every thing that gives them life and motion?

Another method is that of ſkinning birds; they had no other way in Germany and Holland, and it was generally practiſed in France till very lately, when the method of preſerving by allum, ſalt and pepper, was publiſhed and recommended, of which I have already given my opinion. Skinning, compared with the other methods which I have mentioned, is no bad way, but yet it is ſubject to many objections: 1ſt, there is a great difficulty in ſkinning, eſpecially ſmall delicate birds, killed perhaps by large ſhot; 2d, moſt people will find it hardly poſſible to reduce the ſkins to their natural proportions and attitudes, particularly the necks which are often twice as long, when ſeparated from the vertebræ as before; 3dly, the fleſh and bones of the wings and rump muſt, after all, be left with the ſkin, and are as difficult to preſerve as any other parts of the body. However, thoſe who chuſe to continue this method will find their intereſt in making uſe of the materials I ſhall recommend by and by, when I come to treat of my method of preſervation, which I apprehend is more perfect than any of thoſe which I have mentioned, not only in point of preſervation, but alſo as it keeps the natural plumpneſs and proportions. Theſe particulars I ſhall reſerve for other letters, having already made this too long, which I yet muſt encreaſe by begging the candour of the Society for both, as I am writing in a language of which I have very little knowledge. I am,

Learned Sir and Genlemen,

Your moſt obedient,

and very humble ſervant,

London, May 22, 1770. **T. S. Kuckahn.**

LETTER II.

Sir and Gentlemen,

Read June 21, 1770. IN my former letter to the Society, I took the liberty to make ſome obſervations on the common methods of preſerving birds; in this, and two ſucceeding letters, I ſhall endeavour to give a plain deſcription of my method, previouſly requeſting the candid indulgence of the learned Society, for the many inaccuracies which muſt inevitably ſlip from the pen of a foreigner, when writing in a language he is ſo little acquainted with. Before I deſcribe the liquor and other materials which I make uſe of in preſervations, it will be proper to premiſe ſome cautions and give ſuch directions as are neceſſary for preparing the birds that are to be preſerved.

Thoſe who ſhoot birds for that purpoſe, ſhould always be provided with a quantity of cotton or fine tow, with which to ſtop the ſhot holes, and alſo the throat of the bird, to prevent the blood from fouling the feathers, which infallibly ſpoils them. If the birds are not quite killed by the ſhot, they ſhould be immediately diſpatched by preſſing the thumb nail hard upon the wind-pipe, and care ſhould always be taken to confine the wings as ſoon as poſſible, to prevent their fluttering. The birds when dead are to be carried by the legs, and not be crammed into nets or held by the neck, in which laſt poſition the weight of the body would ſtretch it beyond the natural proportion. When they are brought home, they ſhould be hung up by the legs, and the ſtop of cotton taken carefully out of the throat, and a ſmall ſtick put acroſs between the bill, to keep it open, that the blood and ſlime may be diſcharged by the mouth without damaging the plumage. It is alſo neceſſary to obſerve the proper ſeaſons when the birds are in the beſt condition for preſervation, and when not: during the time of incubation, the breaſts and bellies are without feathers, and the ſkin of thoſe parts is extremely tender; again, while birds are moulting or caſting their feathers, they are not fit for preſervation, the quills are full of blood, and the plumage not of its proper colours. The beſt ſeaſons are in the Spring and Autumn; but, if we meet with rare birds, we muſt not loſe the opportunity at any ſeaſon, but do as well as we can.

Young birds are not proper for preſervation till the ſecond year, becauſe they do not, till then, acquire their proportions and colours, which may occaſion their being miſtaken for other ſpecies; neither is it always poſſible in the firſt year, to diſtinguiſh the ſex of birds, which is very eaſy afterwards when they arrive at maturity; however, by grouping young birds in their neſts, we may preſerve them at any time, and when managed in that way they certainly add greatly to a collection.

This naturally leads me to what, in my opinion, is by much the moſt ingenious and entertaining part of this kind of ſtudy, I mean the attitudes and actions of birds; all the reſt is merely mechanical, but this admits of fancy, taſte, and judgment. Without a proper attention to this, however ſound your preſervation, how vivid ſoever the plumage may be, your birds are ſtill nothing but meer dead birds; but by a ſkilful management of attitudes and actions, you, as it were, animate them, they ſeem alive, moving and acting, we

"Liſten attentive, wond'ring, that no ſound
"Eſcapes the buſy throng."

Though this part certainly depends in a great degree on taſte and judgment, yet an accurate obſerver of Nature will derive much information from noticeing the appearance of living birds, in the attitudes and actions which he wiſhes to expreſs in his preſervations: the moſt pictureſque attitude ſhould be fixed upon, and propriety obſerved in chuſing ſuch as are moſt expreſſive of the particular qualities of each bird, as ſtrength and courage in eagles and hawks, &c. In grouping birds of theſe kinds with their prey, regard ſhould be had to the particular part at which they begin to devour it: ſome begin at the breaſt, ſome at the head, ſome at the back, and others extract the entrails firſt; the feeble ſcarce-reſiſting efforts and extreme terror of the proſtrate bird, the exulting audacity and triumph of the victorious one, if properly managed, create a fine contraſt. Picking, ſtretching, feeding, fear, ſurprize, and fighting, afford peculiar and ſtriking attitudes. I fear the word *attitude* does not ſufficiently expreſs my idea; I mean the particular poſitions of the legs, wings, head, body, the manner of the feathers, and in general whatever contributes to expreſs and mark a particular action and paſſion of the bird. Thus, in ſurprize attended with fear, the legs are extended, the body leans forward out of the equilibrium, ſupported almoſt on the toes, the wings are half expanded, the bill turned to one ſide, the top (if creſted) ſpread, and the feathers, particularly thoſe of the neck, ſtanding perpendicular to the ſkin. When any part is not made to co-operate in the expreſſion, we not only loſe the additional ſtrength which the proper action of that part would have given to the general expreſſion, but, what is worſe, the poſition of ſuch deficient parts may convey an idea directly contrary to that general expreſſion, and ſo make the whole unnatural, contradictory, and ridiculous. It is not unfrequent to ſee this abſurdity in a degree that at once ſurprizes and offends a judicious obſerver. Birds put in ſuch poſitions as are intended to expreſs the ſtrongeſt emotions and paſſions, with their feathers perfectly ſmooth and unaffected,

> " Rage with unruffled plumes and fear with falded
> " wings,"

and this abſurdity is the more ſtriking, and therefore the leſs excuſeable, as the action of the wings and feathers are more intelligible and expreſſive than thoſe of any other parts in birds. Great attention ſhould always be had to the poize of the body: in ſuch poſitions as a live bird may be ſupoſed to continue ſome time in, we muſt take care that the body appears in equilibrium; on the contrary, in fighting and other violent actions, where a forceable motion is to be given, the appearance of equilibrium muſt be as carefully avoided, for it always conveys the idea of ſtillneſs, as do the legs when placed by each other, and in the ſame ſtraight direction, which they ſhould ſeldom if ever be in. Bending, advancing, or retiring, one leg a little more than the other, not only gives a more graceful but a more lively and and active appearance; and it is obſervable that living birds, ſtanding on a plain ſurface, almoſt always turn the foot of the leg on that ſide to which they are looking in the ſame lateral direction with the head. I cannot help obſerving here one fault very common with moſt preſervers; that is, the ſtretching the legs of their birds down ſo as to bring the thigh almoſt perpendicular, which not only gives the bird an ungraceful but an unnatural appearance; for we ſeldom or never obſerve this in living birds, except in ſome particular ſpecies.

Birds appear to great advantage when picking their feathers; the tail is then expanded, the wing on that ſide to which the bill is turned lifted up, the other drooping down, and ſomewhat extended from the ſide, in order to balance the body. Birds when fighting afford endleſs variety of attitude and expreſſion; but certainly never any ſo affecting as when grouped with, and feeding their young, whoſe clamorous hunger, expreſſed by their gaping mouths and extended pinions, occaſion that anxious perplexity and tender joy of the mother bird, ſo ſtrongly marked by the ſpreading tail, the drooping wings, and peculiar poſition of the head.

Having now ſet down all that I have to ſay on theſe matters, I ſhall, in my next letter, go immediately upon the method of preparing and applying the materials which I uſe in preſervations. I am,

Sir and Gentlemen,

Your moſt obedient humble ſervant,

May 31, 1770. T. S. Kuckahn.

LETTER III.

Sir and Gentlemen,

Read July 5, 1770. I NOW proceed to deſcribe the materials which I uſe, in preſerving birds, beaſts, &c. and the methods of preparing them, *viz.*

For

For the liquid varniſh, take raw turpentine	2 pounds
camphor	1 pound
ſpirit of turpentine	1 quart

Break the camphor into very ſmall pieces, and put the whole into a glaſs veſſel open at top, place it on a ſand heat till thoroughly warmed, then increaſe the fire gradually till the ingredients are perfectly diſſolved and mixed, which will be done in about half an hour. Great care muſt be taken that the materials do not catch fire: to prevent accidents, it would be better (eſpecially where the proceſs is made in the houſe) to place the glaſs veſſel in another of any metal, two thirds filled with cold water, which place over a gradual fire till it boils, keep it ſo, till the ingredients are diſſolved and incorporated; then take the glaſs off and let it ſtand to cool, and the liquor will be fit for uſe. This varniſh is the only liquid which I uſe in making preparations.

For the dry compound take, *viz.*

corroſive ſublimate	¼ pound
ſaltpetre prepared	½ pound
alum prepared	¼ pound
flowers of ſulphur	½ pound
muſk	¼ pound
black pepper	1 pound
tobacco ground coarſe	1 pound

Mix the whole well together, and keep it in a glaſs veſſel, ſtopped cloſe and in a dry place. To prepare the alum, place it on an iron plate over a fire, till it ceaſes boiling and becomes dry and hard; then take it off, and when cool pulverize it. This method evaporates the aqueous parts of the alum, and alſo renders it much leſs corroſive. The method of preparing ſaltpetre is the ſame as that of alum; only the plate on which it is done muſt have an upright rim all round it, to prevent the ſalt running off into the fire.

In my next letter I ſhall go through the method of diſſecting birds and other ſubjects, and give directions for applying the varniſh and compound, with whatever elſe appertains to a perfect preſervation.

I am, with great deference and reſpect,

Sir, and Gentlemen,

Your moſt obedient humble ſervant,

28 June, 1770.

T. S. Kuckahn.

LETTER IV.

Sir, and Gentlemen,

Read July 5, 1770.

IN my laſt, I gave directions for preparing the materials, which I uſe in preſervations. I ſhall now proceed to deſcribe the method of applying them. If a bird is killed by hand, a ſtop of cotton muſt be put into the throat; and if by ſhot, into each of the ſhot holes alſo, for reaſons heretofore mentioned; then lay the bird on its back upon a table covered with ſeveral folds of ſome ſoft cloth; ſeparate the feathers of the breaſt and belly very carefully, ſo that you may come at the ſkin, in which, about the middle of the breaſt, make an inciſion juſt big enough to introduce the end of a quill-barrel, which enter and blow ſtrongly through, until the ſkin is entirely detached from the fleſh. Continue the inciſion down along the belly to the anus, and contrary-wiſe up to the craw; double back the ſkin on both ſides, carefully guarding the plumage with cotton to prevent its being ſoiled during the operation; and take out the craw. This done, run a ſharp ſmooth ſkewer croſs-ways through the breaſt, and lifting the bird up by it with the left hand, introduce with the other, one point of a ſharp ſtrong pair of ſciſſors cloſe by the edge of the breaſt-bone, and clip along by it, till the breaſt, together with the fleſhy parts of the belly, are entirely ſeparated, taking great care not to cut the inteſtines. Theſe muſt be next extracted, and all the blood and other moiſture dryed up with cotton, ſpunge, or tow, with which the cavity of the body is to be filled. Then draw down the neck within the ſkin until you can come to the back of the ſkull, out of which cut a ſmall piece, and extract the brains; and having dried the cavity well with cotton, dip a hair pencil in the liquid varniſh and waſh it well therewith, and over it ſtrew ſome of the dry compound and fill it up with cotton. Next apply the liquid to the outſide of the ſkull cloſe down to the root of the bill, and over that alſo ſtrew ſome of the powder; proceed in the ſame manner with the neck, and then draw the ſkin back to its proper place, having firſt moiſtened it on the inſide with the liquid.

We now proceed to the wings, the boney parts of which muſt be drawn ſo far on the inſide of the ſkin as that we may come at the whole length. Cut out the moſt fleſhy parts, or only make ſome longitudinal inciſions into them, and apply the liquor and powder as before; then connect the two wings by ſmall wire or ſtrong thread well waxed; then (having removed the cotton that was put into the cavity of the body to imbibe the moiſture) proceed in the ſame manner with the thighs, obſerving, if you cut away the fleſh, to ſupply its place with cotton moiſtened with the varniſh. In order to cure the rump, make as many inciſions in it as may be, without weakening it too much, and having applied the materials as in the other parts, a ſharp wire muſt be run into it, and continued along the under ſide of the back-bone, to about two thirds of the length of the body, in order to ſupport the tail; then, with a pencil, varniſh over the back and inſide of the ſkin, and apply the powder. Stuff afterwards the cavities of the craw and body with the following herbs, *viz.*

Tanſy
Wormwood
Hops and Tobacco } of each an equal quantity well dryed,

and cut ſmall. The next thing is to take particular notice of the breaſt. Out of any ſoft free wood, cut an artificial one as near the ſhape of it as poſſible; which being fitted to its proper place, and moiſtened

with

with the varniſh, muſt be overlaid with cotton ; and the ſkin be drawn over it, being firſt varniſhed on the inſide. In ſewing up the inciſion, obſerve to ſtick the needle always outwards; as you proceed, moiſten the ſeam with ſome of the liquid ; and when finiſhed, diſpoſe the feathers into their natural order. The eyes, muſt be extracted, as no art can preſerve them, ſo as to look full and lively, for the aqueous humour will dry up and of conſequence the outward tunica become ſhrivelled and without luſtre. In extracting them, great care muſt be taken that none of the humour drop on the plumage, as it would ſpoil wherever it touched ; the beſt way is, to ſtick a ſharp pointed awl through each of them and pluck them out together. They muſt be laid aſide in order to finiſh the artificial eyes by. Chuſe for that purpoſe beads of as large ſize as you can conveniently introduce into the orbits ; take a long ſlender needle threaded with ſtrong ſilk waxed over, and run it through the hole in the upper part of the mouth and out at one of the eyes, leaving three or four inches of the ſilk hanging out at the bill. This done, put one bead on the thread, and run the needle out at the other eye ; draw the bead into the orbit, at the ſame time lifting up the eye-lid with a ſharp needle, and place it over the edge of the artificial eye in a natural poſition ; then, with a pencil introduced from the other ſide, varniſh all the cavity with the liquid, and fill the ſpace between the eyes with cotton, ſo as to keep the bead already placed, in its proper place. Put on afterwards the other bead, and returning the needle through the orifice in the upper part of the mouth, draw in the other eye, to its proper orbit, lifting up the lid as before. If the eyes are not ſufficiently protuberant, you may introduce more cotton by the orifice, through which the threads lead; and when you have by this means fixed the eyes properly, tye the ends of the ſilk, and cut them off. There is another method of ſetting the eyes, which is by introducing the beads by the orifice in the roof of the mouth, and when they are placed, ſtuffing cotton through the ſame paſſage to keep them firmly in their places. The ſtop of cotton muſt now be taken out of the throat, and ſome of the ſame material thruſt down very carefully by little at a time, with a quill, to ſupport the neck in its plumpneſs when it becomes dry. We now come to the methods of placing and retaining the birds in the attitudes we would have them; and firſt, we muſt provide the legs with wires ſufficient to ſupport the weight of the body, which is done in this manner. Take a braſs or iron wire of a proper thickneſs, and made ſharp at the point; which run through the foot up the leg and thigh, through the cavity of the body, on the inſide of the wooden breaſt, and ſo up the neck, and out at the upper part of the head, juſt above the bill. The point being then made very ſlender, and turned back like a hook, take hold of the other end of the wire below the foot, and draw it back till the hooked point has fixed in the head, and by it you may adjuſt the length and poſition of the neck and head. (N. B. the wire which is put through the other foot and leg, &c. need not extend to the head, half way along the body will be ſufficient). Next prepare a piece of wire for ſupporting the tail; this muſt be about two-thirds the length of the whole body; ſharpen it at one end and bend the other like a hook, run in the ſharp end juſt below the rump, and puſh it along under the back-bone till the hook is firmly fixed over the rump, amongſt the large feathers of the tail. The next thing is, to fix the bird on the perch or branch, on which you would have it ſtand; in this you will make two holes at the diſtance you propoſe the feet to be, and after having inſerted the wires which are run through the feet and legs, bend the legs and every other part into the attitude you would have them. The wings muſt alſo have a wire to themſelves, in order to keep them in the deſigned poſition; this is done by ſharpening the wire at one end and running it firſt through one wing and through the body, out at the other wing, both being in their proper places: then the feathers muſt be diſpoſed in the manner moſt proper to the poſition of each part, and the expreſſion intended to be conveyed. The feet and bill may be varniſhed over with the ſame ſort of varniſh that is uſed for the preſervation. The bird muſt then ſtand for a day or two in an airy place for the varniſh to penetrate and fix; and laſtly, the bird muſt be baked in an oven; it is not abſolutely neceſſary, but as it makes them dry, and finiſhes the preſervation immediately: moreover if the bird has been ſome time dead and has any diſagreeable ſmell, this method makes it perfectly ſweet; but care muſt be taken not to put them into the oven while it is too hot, as it would bliſter the bill and nails. The beſt rule to know when the oven has a proper degree of heat is this: while the oven is cooling, throw in now and then a tail feather taken from any fowl, which muſt be placed about the middle of the oven. If it is too hot, the feather will have a motion and be bent: we muſt therefore wait a while, and put in another feather, till we obſerve there is no motion or bending; then upon taking it out, and bending it with our finger, if it breaks, the oven is ſtill too hot, and we muſt wait till feathers that have been in for a few minutes will bend without breaking. When the oven is thus fit, the birds muſt be put in, and the door of the oven cloſed, till it is quite cooled. The birds in this manner will be perfectly preſerved; but as there ſtill remains ſome oily matter in the feathers, the moths and other inſects will depoſit their eggs and generate their young in the plumage, if the birds are not carefully caſed up. The caſes muſt be firſt well waſhed on the inſide with the following camphorated ſpirits, *viz.* Take one pound of camphor and boil it in half a gallon of ſpirit of turpentine till well diſſolved; and while hot, waſh all the inſide of the caſes by means of a bruſh, and, as ſoon as dry, place your birds in, and cloſe it up, and guard the joints of the doors or ſeams with paper or putty. N. B. Though the room, in which the caſes of birds, &c. are kept, cannot be too dry, the ſun ſhould not be permitted to ſhine in, as it will certainly diſcharge the finer colours of the plumage.

Baking is not only uſeful in freſh preſervations, but will alſo be of very great ſervice to old ones, deſtroying the eggs of inſects; and it ſhould be a conſtant practice once in two or three years, to bake them over again, and to have the caſes freſh waſhed, as above, which would not only preſerve collections from decay much longer, but alſo keep them ſweet.

I ſhall probably find ſome other opportunity to communicate my method of preſerving quadruped animals, fiſhes, and the halves of birds, for pictures.

I am, with great deference and reſpect,

Sir, and Gentlemen,

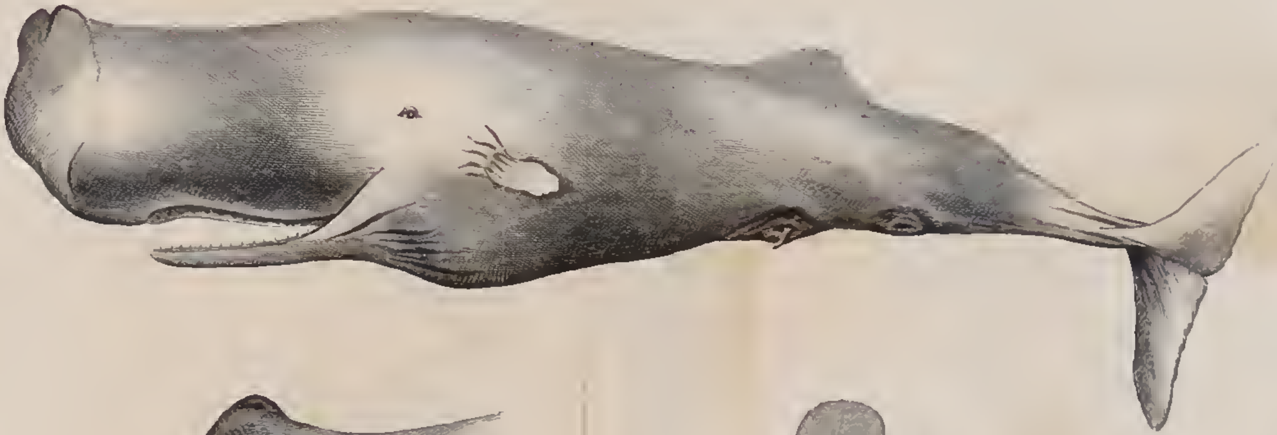
Your moſt obedient

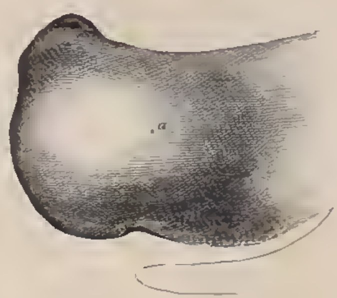
humble ſervant,

London, July 5, 1770.

T. S. Kuckahn.

Philos Trans Vol LX TAB. IX p 321.





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Received March 10, 1770.

XXVII. *Deſcription of the blunt-headed* Cachalot: *By* James Robertſon, *Eſq; of* Edinburgh. *Communicated by* Thomas Pennant, *Eſq; F. R. S.*

Read May 10, 1770.

PHYSETER Catodon Linnæi, blunt-headed Cachalot, Britiſh Zoology, run aſhore upon Cramond Iſland, and was there killed, December 22, 1769. Cramond Iſland is in the Firth of Forth, four miles above Leith. The fiſh meaſured fifty-four feet in length; its greateſt circumference, which was a little behind the eyes, thirty.

The head was nearly one half the whole fiſh, Tab. IX. of an oblong form, and rounded, except within ſix feet of the extremity, where it had inequalities, ſhewn by the tranſverſe ſection (*b*).

The body was rounded, and gradually tapered to the tail, except about the middle of the back oppoſite to the *penis*, where there was a bump or protuberance, but no fin.

The tail, as in all the whale tribe, was placed horizontal a little forked; the blades were of a wedge ſhape, and fourteen feet from tip to tip.

In the lower jaw, which was eleven feet long, were placed twenty-three teeth on each ſide, each two inches long, and all pointing a little outwards.

The upper jaw, projecting five feet over the lower, was quite blunt or truncated, nine feet high; and the ſpout-hole, placed at its upper part, appeared to be provided with a *ſphincter*. In the upper jaw, were twenty-three ſockets on each ſide, for lodging the teeth of the lower, when the mouth was ſhut; but no teeth.

The eyes were remarkably ſmall in proportion to the ſize of the animal, and placed in the moſt prominent part of the head.

The pectoral fins were placed five feet behind the corners of the mouth, and meaſured three feet in length and eighteen inches in breadth.

The *penis* was ſeven feet and a half long and placed nineteen feet behind the corners of the mouth, incloſed in a ſtrong ſheath, the mouth of which was ſhut with a *ſphincter*: five feet behind it was placed the anus, likewiſe furniſhed with a *ſphincter*, and the diſtance, from the *anus* to the diviſion in the blades of the tail, was fourteen feet.

The *cuticula*, or ſcarf-ſkin, was extremely thin; on the upper part of the head and whole body, of a bright grey colour, and on the under part of the head of a dirty white; it was ſmooth and ſlippery to the touch, eaſily torn off, and when viewed betwixt the light, it appeared ſcaly.

The true ſkin was of a black colour, about $\frac{1}{4}$ of an inch thick, adhering firmly to the fat (blubber), which was from four to nine inches thick. Below the fat every where were tendinous cords, of a bright ſtraw colour, very elaſtic, ſtrong, and covered with a looſe thin membraneous coat. On the *abdomen* were three layers of thoſe tendons, which croſſed each other obliquely, and, in their direction and ſituation, greatly reſembled the *obliquus aſcendens* and the *tranſverſalis* of the human body, and they became fleſhy where the *linea alba* is in the human body, and at the lumbal vertebræ. The tendons which appear to ariſe from the upper ribs, the dorſal *vertebræ*, and the *vertebræ* of the neck, aroſe fleſhy, were both flatter and ſtronger than in any other part of the body, and running along the head with little obliquity, ſeemed to be inſerted tendinous into the *cranium*, &c. Conſidering the tail as the *os ſacrum* or a continuation of the ſpine, the tendinous muſcles belonging to it aroſe towards the proceſs of one *vertebra*, and running almoſt round, was inſerted into the proceſs of another, and have much the ſame effect on the tail as the *ſupinator* and *pronators* have in turning the hand; which circumſtance, if true, muſt be of great utility in performing the ſeveral motions neceſſary in the progreſſion of this animal.

† The ſubſtance, improperly called *ſpermaceti*, and erroneouſly ſaid to be prepared from the fat of the brain, was every were contained in a fluid ſtate in the cavity of the head along with the brain, but quite diſtinct from it. Was this ſubſtance in a ſtate of fluidity when the animal was in life? Very probably not; but it turned into that form by means of a heat occaſioned by a fermentation of the different fluids, which ſoon began after the death of the fiſh, and increaſed to ſuch a degree as at length to cauſe many cracks in the ſkin, to burſt the body in the back, and to

† v. Ph. Trans. LXXII p. 40 —

throw out the abdominal *viſcera* at that aperture. After this eruption the *ſpermaceti* was found every where around the fiſh, floating on the water in a congealed ſtate. From which circumſtance, it ſeemed to be contained throughout the whole body, and to have run out at theſe cracks, &c.: but upon examination, it was found to have run out at the mouth only. How found it a paſſage from the head there? To come at that fluid, the workmen made a hole into the cavity of the head at (*a*), and took it out with a ſkimmer from among the ſubſtance of the brain, as it flowed to the hole, which it did like water ſpringing up into a well. When it was taken out, it was hot, and of a clear oily colour; but being expoſed to the cold air, it immediately congealed into a white maſs.

XXVIII. *Experiments and Obſervations on various Phænomena attending the Solution of Salts: By* R. Watſon, *A. M. F. R. S: Fellow of* Trinity College, *and Profeſſor of Chemiſtry, in the Univerſity of* Cambridge.

Read May 24, 31, 1770.

HAVING lately had occaſion, in ſome chemical enquiries, to make various ſolutions of ſalts, I met with ſome phænomena, which did not appear to me either to have been ſufficiently attended to, or conſiſtently explained by writers upon that ſubject. The ſuſpenſion of ſalts in water, of metals in acids, of ſulphur in oils, and of other bodies in menſtruums ſpecifically lighter than the bodies themſelves, hath ever been conſidered in chemiſtry, as a problem of difficult ſolution. Thoſe philoſophers who acquieſce, upon the whole, in the cauſe which hath been aſſigned for this phænomenon by Sir Iſaac Newton, in his optical Queſtions, have taken great pains to illuſtrate the manner how it is effected, by ſuppoſing that the bodies are received into the pores of their reſpective menſtruums, and there kept ſuſpended by the attraction or, as Bernouilli and Freind would have it, by the reſiſtance ariſing from the tenacity of the fluid. Hence it happens, ſay theſe philoſophers, that after water is ſaturated with one ſalt, it is ſtill capable of diſſolving ſomewhat of a ſecond kind, and being ſaturated with that, of a third, and ſo on; juſt as a veſſel filled as full as poſſible with ſpheres or cylinders of one magnitude hath a capability of receiving ſimilar bodies of an inferior ſize, or bodies of a different figure. The opinion of Gaſſendus ſeems to have been generally adopted; he endeavours to prove, from the experiment which hath been mentioned, not only the poroſity of water, but a diverſity in the figures of the pores: *Affero & aliud experimentum ſingulare, quo viſus ſum mihi deprehendere interſperſa hujuſmodi ſpatiola inania intra aquam dari.—Aiebam, cum ſint ſalis corpuſcula cubica, poterunt ea quidem replere ſpatiola, quæ & ipſa cubica fuerint; at cum non modo commune ſal, ſed alumen etiam, quod eſt octahedricum, halinitrum item, & ſal ammoniacum ſaccharumque & alia quæ aliarum ſunt figurarum eâdem aquâ exſolvi poſſunt; erunt ergo etiam in aqua ſpatiola octahedrica atque id genus alia; adeo ut aqua, tametſi ſale ſaturata fuerit, nihilominus & alumen et cetera omnia exſolvere poſſit ac in ſeſe transfundere.* Gaſ. Phyſ. l. i. ſect. 1. cap. iii. The reaſon why warm water diſſolves in general more ſalt than cold water, ſeems as if it might be derived from the ſame principle, was it true; the interſtices between the elementary particles of water are enlarged by the expanſion of the fluid, and might therefore be ſuppoſed capable of admitting into them a larger quantity of ſalt. This doctrine hath been embraced by moſt philoſophers, eſpecially by the late Abbé Nollet, in the 4th volume of his Leçons de Phyſique; and I do not know that it hath been oppoſed by any body. The late Mr. Eller, of Berlin, hath carried this ſpeculation ſo far, as to publiſh a Table in the Berlin Memoirs for 1750, exhibiting the ſeveral quantities of above twenty different kinds of ſalt, which a given quantity of water will abſorb into its pores, without being in the leaſt augmented in bulk. It is not therefore without ſome uneaſineſs that I find myſelf conſtrained to diſſent from the general opinion, and particularly to differ from Mr. Eller, who hath treated this ſubject *ex profeſſo*; who made his experiments, as he himſelf aſſures us, with the greateſt exactneſs; and who was led by them to the diſcovery of what he is pleaſed to call, *une verité inconteſtable, ſavoir, que les plus petites parties conſtituantes de l'eau ſont doüees de pores ou d'interſtices dans leſquels les atomes de ſel peuvent nicher, ſans augmenter leur volume.* I do not at preſent ſee any very probable method of reconciling the different reſults of our enquiries; I will therefore content myſelf with giving a plain relation of the experiments which I have made upon this ſubject.

EXPERIMENT I.

I took a large mattraſs, containing, when filled to the middle of its neck, 132 ounces of water, Troy weight; the diameter of the cavity of the neck was ſix lines: having with a diamond marked the place where the water ſtood in the neck of the mattraſs, I dropped into it a ſingle piece of purified nitre, the weight of which was a 2600th part of the weight of the water, and immediately obſerved that the water was conſiderably elevated in the tube: during the ſolution of the ſalt, the water ſunk near one third of its whole elevation; but when the ſolution was entirely finiſhed, it remained very ſenſibly raiſed above the mark: ſo that, even from the experiment with this inſtrument, we may be aſſured that water cannot abſorb $\frac{1}{2000}$th part of its weight of nitre, without being augmented in bulk. Mr. Eller, from his experiments, concludes, that eight ounces of water will abſorb one drachm and a half, or above a 42d part of its weight of nitre; and hence I ſuppoſed the quantity of water which I uſed would have abſorbed above ſixteen times as much, or above 3 ounces; whereas the event ſhewed that it could not abſorb $\frac{1}{20}$ of an ounce. From the ſinking of the water during the ſolution, I was at firſt inclined to believe that ſome part at leaſt of the nitre was taken into the pores of the water: in order to ſee whether this conjecture could be verified by fact, I made the following experiment.

EXPERIMENT II.

I choſe two mattraſſes of unequal ſizes, containing quantities of water in the proportion of 12 to 1, the diameters of the necks being equal: into the largeſt I put $\frac{1}{650}$th part of the water's weight of nitre, and an equal quantity into the ſmaller; and I obſerved that the water, as well before as after the ſolution, was equally elevated in them both: this experiment was repeated. Now, if a given quantity of water can abſorb into its pores, without being increaſed in magnitude, any quantity of ſalt however ſmall, it ſeems reaſonable to ſuppoſe that a quantity containing twelve times as many pores ſhould abſorb twelve times as much, (ſince it is an allowed fact that the minuteſt portion of a ſalt is uniformly diffuſed through the largeſt quantity of water) and it might conſequently be expected, that the water ſhould riſe higher in the neck of the ſmaller matraſs than in that of the larger, which is contrary to the experiment.

EXPERIMENT III.

Apprehending that common pump water, with which I had made the preceding experiments, might have its interſtices preoccupied by ſelenites and other heterogeneous matters, and be thereby rendered incapable of admitting into them any additional ſubſtance; and obſerving that Mr. Eller had uſed in all his experiments 8 ounces of diſtilled water, I had hopes to have reconciled my experiments to his by that means: but upon trial, with diſtilled water, I found the elevation preciſely the ſame as before. Nor do the concluſions depend upon the kind of ſalt; they hold true *mutatis mutandis* of any other ſalt as well as nitre. During the ſolution the water is refrigerated and thereby contracted in magnitude, and the ſmaller the quantity the greater will be the cold and conſequent contraction produced by the addition of ſmall portions of ſalt; but I cannot ſuppoſe that this circumſtance could be overlooked by Mr. Eller, though it induced me to uſe a much larger quantity, or that he attributed the ſinking of the water during the ſolution, to an imbibition of the particles of the ſeveral ſalts into the pores of the water, and thence by calculation conſtructed his table.

EXPERIMENT IV.

Having always remarked that the water in the neck of the matraſs was elevated higher upon the firſt immerſion of the ſalt, than after it was wholly diſſolved, I endeavoured to aſcertain the difference in ſeveral kinds of ſalt. To do this with the greater exactneſs, I pitched upon a matraſs which had a neck as far as I wanted it accurately cylindrical, as I found by obſerving the elevations occaſioned by the additions of equal portions of water; the matraſs held about 67 ounces of water. The ſalts I uſed were all dry, and in as large pieces as the neck of the matraſs would admit; the water was heated to the forty ſecond degree of Fahrenheit's thermometer, and kept as nearly as could be in that temperature. I changed the water for each experiment, and uſed in each 24 penny weights of ſalt; the heights to which the water roſe, as meaſured from a mark in the middle of the tube, before and after the ſolution of each ſalt are expreſſed in the following table: the firſt column denotes the height to which the water was elevated by 24 penny weights of ſalt before its ſolution, the ſecond after its ſolution, the third the difference in fractional parts of the elevation before ſolution

Elevation by 24 penny weights of ſimple water	0	58	
24 penny weights of genuine Glaubers ſalt	42	36	$\frac{1}{7}$

Vol. ſalt of ſal. ammon.	40	33	$\frac{7}{40}$
Sal ammon.	40	39	$\frac{1}{40}$
Refined white ſugar	39	36	$\frac{1}{13}$
Coarſe brown ſugar	39	36	$\frac{1}{13}$
White ſugar candy	37	36	$\frac{1}{37}$
Glaubers ſalt from Lymington	35	29	$\frac{6}{35}$
Terra foliata tar.	37	30	$\frac{7}{37}$
Rochelle ſalt	33	28	$\frac{5}{33}$
Alum not quite diſſolved	33	28	$\frac{5}{33}$
Borax not half diſſolved in 2 days	33	31	$\frac{2}{33}$
Green vitriol	32	26	$\frac{3}{16}$
White vitriol	30	24	$\frac{1}{5}$
Nitre	30	21	$\frac{3}{10}$
Sal gem. from Northwich	27	17	$\frac{10}{27}$
Blue vitriol	26	20	$\frac{3}{13}$
Pearl aſh	25	10	$\frac{3}{5}$
Vitriolated tartar	22	11	$\frac{1}{2}$
Green vitriol calcined to whiteneſs	22	11	$\frac{1}{2}$
Dry ſalt of tartar	21	13	$\frac{8}{21}$
Baſket ſea ſalt	19	15	$\frac{4}{19}$
Corroſive ſublimate	14	10	$\frac{2}{7}$
Turbith mineral	9	0	

Had I not been in ſome meaſure perſuaded, from the reſult of the preceding experiments, that no portion of any ſalt could be abſorbed into the pores of water, I ſhould have readily concluded that the third column of this table denoted ſuch parts of 24 penny weights of the ſeveral ſalts as might be lodged in the interſtices of 67 ounces of water without increaſing its magnitude: the quantities indeed which might have been thus aſcertained would have but ill agreed with thoſe which are determined by Mr. Eller; and that diverſity of quantity may ſuggeſt a doubt concerning the validity of his principle. The ſinking of the water in the neck of the matraſs ſeems to be a general phænomenon attending the ſolution of all ſalts; the quantity of the deſcent is various from $\frac{1}{40}$ to $\frac{1}{2}$ of the whole elevation in thoſe ſalts which I have tried. In forming the table, I repeated many of the experiments, but found no variation which could affect the general concluſion; with particular attention I repeated the ſolution of vitriolated tartar, for I thought it a very remarkable citcumſtance that one of the hardeſt ſalts ſhould be more diminiſhed in proportion to its whole bulk than any other, but the numbers in the table 22 and 11 accurately expreſſed the height before and after ſolution upon the repetition of the experiment, ſo that it may be relied upon as a certain fact that a cubic inch of vitriolated tartar is by ſolution in water reduced to half a cubic inch, though the water cannot, as appeared from an experiment I made, abſorb $\frac{1}{1000}$th part, nor, as I believe, any part, of that ſalt without being augmented in magnitude. It is evident from the table that ſal gemmæ, blue vitriol, corroſive ſublimate, calcined vitriol, and in general thoſe ſalts which retain the leaſt water in their compoſition and conſtitute the hardeſt maſſes, ſink more in proportion to their reſpective bulks than any other. I own myſelf at a loſs for a general principle to explain this general phænomenon, unleſs the air contained in the ſeveral ſalts may be eſteemed ſufficient for the purpoſe; a very copious ſeparation of air from the ſalts during the whole time of their ſolution may be readily obſerved in all of them, and a ſmall portion of it, combined with the particles of a ſalt, may augment its bulk, without ſenſibly increaſing its weight. Yet the two following experiments rather tend to diminiſh the probability of this opinion.

Experiment V.

I took water which had been well purged from its air by long boiling, and which had been corked up whilſt it was warm; when it had acquired a proper temperature, I filled a matraſs with it, as before, and putting into it ſal gemmæ, &c. I obſerved that the elevation before ſolution was the ſame as when common water was uſed, and that it ſunk equally in the neck during the ſolution; but then the ſeparation of air ſeemed greatly leſs in all the trials I made. This phænomenon is eaſily explained: common water is always ſaturated with air; upon the addition of any ſalt, the particles of water begin to attract and diſſolve the ſalt, and let go the air with which they are united; this air, added to the air contained in the ſalt, renders the whole much more viſible in common than in boiled water. Muſſchenbrook and others are of opinion, that air only fills the interſtices of water, without augmenting its bulk; they ground their opinion upon obſerving that the ſpecific gravities of common water and of water purged from its air are equal; the fact, taking it for granted, will ſcarcely authorize the concluſion: for, ſuppoſing that a cubic inch of common water contains even a cubic inch of air, the difference of the weight of the water when ſaturated with air, and when freed as much as poſſible from it (though probably it can never be wholly freed from it), will not equal $\frac{1}{4}$ of a grain: how imperceptible then muſt the difference be, if water, inſtead of an equal bulk, doth not contain $\frac{1}{160}$th part of its bulk of air, which is a ſuppoſition much nearer to the truth: the air is ſeparated from the water during the ſolution of the ſalt, and the particles of the ſalt probably occupy its place as happens in other chemical precipitations; but we cannot thence infer that they are received into the interſtices of the water, unleſs we had more concluſive arguments, to prove that the air itſelf was lodged in them. I varied the preceding experiment by putting two equal and tranſparent pieces of ſal gemmæ into two tall drinking glaſſes, filled one with common, the other with boiled water; from the firſt there continually aſcended a very viſible ſtream of air, and the ſalt and the bottom of the glaſs were covered with bubbles, it ſeeming as if the water quitted its air to diſſolve the ſalt; in the other, though ſome air was ſeen breaking out from the ſalt whilſt it was diſſolving, there did not ſeem to be any precipitated, as it were, from the water. In moſt of the experiments which I made, the boiled water diſſolved a given quantity of ſalt ſooner than the common water, when they had the ſame degree of heat; but the difference in time might be owing to the different magnitude of the ſurfaces of the ſalt, though from the generality of the event, I ſhould rather attribute it to the different diſſolving powers of water, when replete with, and when deprived of air.

EXPERIMENT VI.

Thinking that the difference in the bulks of the water before and after ſolution might be owing to the ſeparation and eſcape of ſome volatile principle; I took care to balance as accurately as I could, water and ſal gemmæ, water and ſalt of tartar, water and vitriolated tartar, &c. and then putting the ſeveral ſalts into the water, I obſerved when the ſolution was accompliſhed, whether the equilibrium of the ſcales was affected, but I could not diſtinguiſh any change. Dr. Hales and others have ſpoken of the exiſtence of air in ſalts, and have in two or three inſtances inveſtigated the quantity, but after a very different manner from that I have uſed; nor can I think myſelf at liberty to eſteem this air which is ſeparated by ſolution, of the ſame nature with that which is called by him and others fixed air, inaſmuch as fixed air makes a conſiderable part of the weight of the bodies from which it is extracted, precipitates lime water, and is ſeldom diſcharged (or perhaps produced from ſome of the minute parts of the body being converted by the violence of the fire, &c. into an elaſtic fluid), except when the body is decompoſed; whereas this makes only a conſiderable part of the bulk of bodies, and thus diminiſhes their ſpecific gravity without ſenſibly increaſing their abſolute weight; does not, as I collected from ſome rough trials, render lime water turbid; and is ſet at liberty, though not by a mechanical diviſion, yet by an operation ſomewhat different from chemical decompoſition. It hath been remarked by ſome, that ſaline ſolutions will not cryſtallize without much difficulty in an exhauſted receiver; perhaps becauſe the particles of ſalt cannot attract that principle which ſhould cement them together, which at leaſt may be ſeen eſcaping from them when they begin to be ſeparated. Mr. Boyle obſerved, that aquafortis, poured upon a ſtrong vegetable alcali, did not cryſtallize till it had been long expoſed to the air (though I ſhould rather attribute this failure to the weakneſs of his aquafortis than to the want of air, ſince I have frequently, by uſing the fuming ſpirit of nitre, obtained cryſtals of an inch in length almoſt inſtantaneouſly); and ſeveral other phænomena might be adduced reſpecting the cryſtallization of ſalts, which ſeem to indicate the neceſſity of admitting air as a very efficacious inſtrument in producing that effect: but future experience may tend to elucidate this matter. Having uſed great attention in making the experiments from which the preceding table was compoſed; I thought I had a good opportunity of deriving from it the ſpecific gravities of the ſalts which are there mentioned. I accordingly calculated the following table; in the firſt column of which are expreſſed the ſpecific gravities as calculated from the increaſe of bulk before ſolution; in the ſecond, after the ſolution.

Genuine Glauber's ſalt	1,380	1,611
Cryſtals of kelp	1,414	1,467
Volat. ſalt of ſal ammoniac	1,450	1,787
Sal ammoniac	1,450	1,487
Sugar refined, brown, barley	1,487	1,611
White ſugar candy	1,567	1,611

Terra foliata tartari	1,567	1,933
Glauber's ſalt from Lymington	1,657	2,000
Rochelle ſalt	1,757	2,071
Alum	1,757	2,071
Borax	1,757	
Green vitriol	1,812	2,230
White vitriol	1,933	2,416
Nitre	1,933	2,766
Very tranſparent ſal gem. from Nortwich	2,143	3,411
Blue vitriol purified	2,230	2,900
Pearl aſh	2,320	5,800
Vitriolated tartar	2,636	5,272
Green vitriol calcined to whiteneſs	2,636	5,272
Dry ſalt of tartar	2,761	4,461
Baſket ſea ſalt	3,052	3,866
Corroſive ſublimate	4,142	5,800
Mercury diſtilled with acid of vitriol, and freed from its acid by a ſtrong fire	6,444	

The numbers in the firſt column correſpond very well, upon the whole, with the ſpecific gravities which have been determined by others hydroſtatically; thus the ſpecific gravities of nitre, alum, white and green vitriol, ſal ammoniac, ſal gemmæ, &c. are greater than what are aſſigned to theſe bodies by ſome authors, and leſs than what have been determined by others; it ſeems as if the ſpecific gravities of ſaline bodies might, in a proper veſſel, be more accurately aſcertained from the obſerved increaſe of the water's bulk than any other way. Upon the ſuppoſition that the eſcape of the air is the reaſon of the water's ſinking during the ſolution, and that this air contributes little to the weight of the ſalts, though it may be abſolutely neceſſary to the exhibiting the ſaline moleculæ under a viſible cryſtalline appearance; the ſecond column will denote the real ſpecific gravities of the ſalts as freed from air. That this air is combined with the ſalts, and doth not ſimply adhere to their ſurfaces, may appear from hence, that the ſpecific gravities, as calculated from the increaſe of bulk obſerved in the water before ſolution, ſufficiently correſpond with thoſe which philoſophers have determined hydroſtatically: nor indeed, upon exhauſting the air from the ſalts, by an air pump, could I obſerve that it was ſeparated, in leſs quantity during ſolution.

EXPERIMENT VII.

Since equal quantities of ſalt muſt contain equal quantities of air, it might be expected *a priori*, if the eſcape of the air was the occaſion of the water's ſinking, that equal weights of ſalt would produce equal augmentations of bulk, and unequal weights augmentations proportionable to their weights; but, to be aſſured of this, I took a matraſs containing about 30 ounces of water, the tube being cylindrical for about 7 inches in length. When the matraſs was filled to a proper mark, I put into it 7 pennyweights of powdered ſal gem.: the water after the ſolution had riſen through 17 tenths of an inch; by the addition of 14 pennyweights more, the water was raiſed through 51 diviſions from the firſt mark, or twice 17 from where it ſtood after the ſolution of

7 pennyweights. In the ſame matraſs I tried a ſimilar experiment with nitre; the water was raiſed through 10 diviſions, by 3 pennyweights of powdered nitre; and by 18 more, it ſtood after the ſolution at the 70th diviſion from the firſt mark, and conſequently roſe through ſix times the ſpace, through which it had been raiſed by 3 pennyweights. From theſe, and other experiments of the ſame kind, I am diſpoſed to believe that equal portions of ſalt produce equal augmentations in the bulk of the water wherein they are diſſolved; at leaſt, this holds true when the ſalt diſſolved bears but a ſmall proportion to what would be requiſite to ſaturate the water. But, in making this experiment, great care muſt be taken to keep the ſalts of the ſame dryneſs; I had once tried it with three equal quantities of ſea ſalt, and arrived at a quite different concluſion; the increaſes of bulk occaſioned by the ſolution of the ſeveral ſalts being ſeparately taken, as 15, 16, 17, but the ſalt being much drier than the air in the laboratory, had undoubtedly attracted the humidity, and that portion had attracted the moſt which had been the longeſt in it, and which was laſt diſſolved. Nor ſhould the temperature of the water be neglected; a ſenſible error may proceed from a minute change in that. This experiment confirms the firſt, for, was any part of ſalt abſorbed into the pores of the water, it certainly ought to be expected that the elevation occaſioned by the ſolution of 3 pennyweights of nitre ſhould be leſs than $\frac{1}{6}$th of that occaſioned by 18 pennyweights, and yet I found it to be accurately $\frac{1}{6}$th upon repeating the experiment with diſtilled water. It confirms it too in another view, 3 penny-

weights or $\frac{1}{200}$th part of the weight of the water, raiſed it through one inch; hence $\frac{1}{2000}$th part would have raiſed it through one tenth of an inch, which any eye may diſtinguiſh.

Dr. Lewis, for whoſe great abilities in chemiſtry I have a very high reſpect, in his little treatiſe upon American potaſhes, is of opinion, that the augmentation of the bulk of water doth not proceed uniformly, according to the quantity of ſalt added; and he forms his concluſion from obſerving, that the loſſes of weight ſuſtained by the ſame body in different ſolutions, were not uniform, but continually diminiſhed; the loſſes correſponding to ſeven ſucceſſive equal quantities being as 24½. 24. 23½. 22. 22. 21. 20. Upon conſidering this matter in a mathematical light, I an inclined to draw a quite different concluſion; but I will firſt mention ſome experiments which I had formerly made with a different view, and which agree very well with Dr. Lewis's.

EXPERIMENT VIII.

I had conceived that if, in a given quantity of water, ſeveral quantities of ſalt, increaſing in any arithmetical or geometrical progreſſion, were diſſolved; that the increments of ſpecific gravity would increaſe in the ſame progreſſion. In order to ſee whether this conjecture could be eſtabliſhed by experiment, I diſſolved in a given quantity of water, different portions of ſea ſalt, increaſing in the progreſſions expreſſed in the annexed tables, where the firſt column of each denotes the proportional quantities of ſalt in pennyweights, the ſecond, the loſs of weight of a given body in quarter grains; the third the exceſs of the ſpecific gravity of each ſolution, above the ſpecific gravity of water.

TAB. I.

	263	0
9	273	10
18	282	19
27	292	29
36	301	38
45	309	46

TAB. II.

	263	0
5	269	6
10	274	11
15	280	17
20	285	22
25	289	26
30	294	31
35	300	37
40	304	41
45	309	46
50	312	49
55	316	53

TAB. III.

	883	0
4	899	16
8	915	32
12	930	47
16	945	62
20	959	76
24	971	88
28	985	102
32	996	113
36	1009	126
40	1020	137

The difference of the numbers in the third column of each table from arithmetical progreſſions, is obvious at firſt view, the difference of the two laſt numbers of each being conſiderably leſs than the difference between the two firſt: and the numbers 6. 11. 22. 41. correſponding to the geometrical progreſſion 5. 10. 20. 40. in the ſecond table as well as the numbers 16. 32. 62. 113 correſponding to the geometrical progreſſion 4. 8. 16. 32, in the third, differ conſiderably from geometrical progreſſions, whoſe common ratio is $\frac{1}{2}$.

In making theſe experiments there are three obvious ſources of error: the heat may not remain conſtant; the additional weights of ſalt may not be accurately equal; and the weight of the given body may be more or leſs than what is expreſſed by any quantity leſs than $\frac{1}{4}$ of a grain; yet the differences of the preceding numbers, from arithmetical or geometrical progreſſions, are too great to be explained from any or all of theſe ſources taken together. We may obſerve that the loſſes of weight, correſponding to equal portions of ſalt, are, upon the whole, diminiſhed; but it will not follow from thence that the bulks are not equally augmented. For, ſince the ſpecific gravity of every body is properly denoted by a fraction, whoſe numerator expreſſes the abſolute weight, and denominator the magnitude of the body; let $\frac{w}{m}, \frac{w+x}{m+y}, \frac{w+2x}{m+z}, \frac{w+3x}{m+s}$, &c. be a ſeries of fractions, whoſe ſeveral numerators expreſs the weights of a given quantity of water, as increaſed by the addition of equal portions of any ſalt denoted by x, and whoſe denominators expreſs the bulks of the water after the ſolution of each portion of ſalt, the increments of bulk being denoted by y, z, s; now let us ſuppoſe that the loſſes of weight ſuſtained by the ſame body, that is, the ſpecific gravities, increaſe uniformly, then will the above ſeries of fractions increaſe uniformly, let $\frac{w}{m}=a$; $\frac{w+x}{m+y}=a+b$; $\frac{w+2x}{m+z}=a+2b$; $\frac{w+3x}{m+s}=a+3b$, from theſe equations inveſtigating the proportion between y, z, s, which repreſent the augmentations of bulk, it will appear that $y:z::a+2b:2a+2b$, or in a greater ratio than that of $1:2$ and that $z:s::2a+6b:3a+6b$ or in a greater ratio than that of $2:3$, in which ratios they ought reſpectively to have been, had the denominators or the bulks of the fluid increaſed uniformly, when the ſpecific gravities or abſolute weights increaſed uniformly. We ſee from this, what concluſion ſhould have been formed, had the increments of ſpecific gravity from equal portions of ſalt been equal. Again, ſuppoſe that $\frac{w}{m}$, $\frac{w+p}{m+q}$, $\frac{w+2p}{m+2q}$, $\frac{w+3p}{m+3q}$, &c. denote a ſeries of fractions, whoſe numerators, expreſſing the weights of a given quantity of water as increaſed by the addition of ſalt, and whoſe denominators, expreſſing the bulks, both increaſe uniformly, then will the ſeveral differences between the 2d and 1ſt, between the 3d and 2d, and ſo on, be as $\frac{1}{m \times \overline{m+q}}$, $\frac{1}{\overline{m+q} \times \overline{m+2q}}$, $\frac{1}{\overline{m+2q} \times \overline{m+3q}}$, $\frac{1}{\overline{m+3q} \times \overline{m+4q}}$, &c. which fractions being inverſely as their denominators conſtitute a decreaſing ſeries; but the increments of ſpecific gravity from the addition of equal portions of ſalt, are proportionable to theſe fractions, and therefore ought perpetually to decreaſe, though we allowed the bulk of the compound to be preciſely equal to the bulk of the water and ſalt taken together, that is, though we allowed the bulk of the water to increaſe uniformly according to the quantity of ſalt added: now as it is evident from Dr. Lewis's experiments, and from each of the preceding tables, that the increments of ſpecific gravity do decreaſe upon the whole, when the abſolute weights increaſe uniformly, we may venture to conclude that the bulks increaſe uniformly alſo. I thought proper to explain the foregoing principle and to determine the ratio, becauſe the matter ſeems to have been miſtaken by many; however, it may be eaſily apprehended that the increments of ſpecific gravity, from the addition of equal quantities of ſalt to a given weight of water, ought perpetually to decreaſe; becauſe the difference between the ſpecific gravities of the water and of the ſalt perpetually decreaſes, as the water approaches to perfect ſaturation. In like manner, if to a given quantity of water we add any number of equal quantities of oil of vitriol, or any fluid miſcible with and heavier than water; the increments of ſpecific gravity will perpetually decreaſe, though they will never entirely vaniſh, becauſe there is a perpetual approximation to the ſpecific gravity of the acid, which yet the mixture can never acquire; and, *vice verſa*, if to water we add a lighter fluid, as ſpirits of wine by equal portions, the ſpecific gravity of the mixture will conſtantly decreaſe by unequal decrements; but the decrements will never vaniſh, becauſe the mixture muſt ever remain ſpecifically heavier than ſpirit of wine.

EXPERIMENT IX.

The quantities of various ſalts, which may be diſſolved in a given quantity of water, have been aſcertained by Boerhaave, Eller, Spielman, and others; their accounts differ ſomewhat from one another, as might be expected from the different temperatures of the air, the different ſtate of their ſalts; the different times (a circumſtance of no ſmall conſideration in this matter) which they allowed the water to act upon the ſalts before they concluded it to be fully ſaturated, and from ſome other circumſtances which might perhaps with advantage be taken into the account, and a more accurate table compoſed than hath hitherto been publiſhed; but as the differences would be ſmall, and might not tend to any new diſcoveries, I could not perſuade myſelf to be at the trouble of making the requiſite experiments. I thought it would be a more uſeful undertaking to determine the ſpecific gravities of ſaturated ſolutions of various ſalts. In compoſing the following table, I uſed every poſſible precaution; the ſolutions were fully ſaturated, by permitting the water to reſt upon the ſalts for ſome weeks, and frequently ſhaking the ſolutions during the interval: I had ſome reaſons for chuſing this method rather than the much ſhorter one of diſſolving the ſalts in hot water, and letting the ſolutions cool, though the event will be much the ſame in both ways; my balance was extremely ſenſible, though I did not uſe any weight leſs than a quarter of a grain; the water in which the ſalts were diſſolved was not ¼ of a grain in 890 heavier than diſtilled water; the ſolutions were all of the ſame temperature, Fahrenheit's thermometer ſtanding between 41 and 42° during the whole time of taking the ſpecific gravities.

A Table exhibiting the ſpecific gravities of water ſaturated with various ſalts. Thermometer 41—42°, barometer 30 inches.

Water in which the ſalts were diſſ.	1,000	Cryſtals of tar	1,001
		Arſenic	1,005
Saturated with quicklime	1,001	Borax	1,010
		Corroſ. ſublim.	1,037

Alum	1,033	Nitre purified	1,095
Genuine Glau. ſalt	1,052	Rochelle ſalt	1,114
Vitriolated tart.	1,054	Blue vitriol	1,150
Common ſalt	1,198	Green vitriol	1,157
Arſen. nitre	1,184	Sal gemmæ	1,170
Glau. ſalt Lyming.	1,232	Epſom ſalt Lym.	1,218
Sal ammon.	1,072	White vitriol	1,386
Vol. ſalt of ſal. am.	1,077	Pearl aſh	1,534
Cryſtals of kelp	1,087		

By making other tables ſimilar to the preceding, when the thermometer ſtands at 62°, 82°, 102°, &c. or when the heat increaſes or decreaſes in any known ratio; it is extremely probable that the law, according to which the diſſolving power of water varies with the variation of its heat, might be inveſtigated. I have ſome reaſons for thinking that though it increaſes with the increaſe of heat, yet it doth not increaſe in the direct ſimple ratio of the heat; but what the law is, or whether all ſalts follow the ſame law, I cannot, from any experiments I have already made, determine; and I have no leiſure at preſent to proſecute the enquiry. The concluſion will be unavoidably liable to a ſmall inaccuracy; for whether the ſpecific gravities be inveſtigated by weighing the ſeveral fluids in a given veſſel filled to a given mark, or by weighing a given ſolid in each of them, we ſhall not thence obtain the weights of equal bulks, ſince the containing veſſel or the ſolid, from the difference of the heats, have a different capacity or a different bulk. However, it is not apprehended that this circumſtance would ſenſibly affect the concluſion, eſpecially as it is ſubject to calculation and might be allowed for. It ought, at the ſame time, to be obſerved, that a given bulk of the water with which the ſpecific gravities are compoſed, will have different weights when the heats are different; and theſe differences ought firſt to be aſcertained.

EXPERIMENT X.

Having thus determined the ſpecific gravities of ſaturated ſolutions of ſeveral ſalts, in a given degree of heat; my next enquiry was to find the ſpecific gravities of water impregnated with a given quantity of the ſeveral ſalts: I accordingly diſſolved in 168 pennyweights of water, 14 pennyweights, or $\frac{1}{12}$ of the weight of the water of the eight following ſalts. The thermometer was at 40° and barometer at $29\frac{1}{2}$.

A Table of the ſpecific gravities of water impregnated with $\frac{1}{12}$ of its weight of

Water	1,000
Sea ſalt	1,059
Blue vitriol	1,052
Nitre	1,050
White vitriol	1,045
Green vitriol	1,043
Glau. ſalts Lym.	1,039
Glau. ſalts Genu.	1,029
Sal ammon.	1,026

I could not have made this table much more extenſive, ſince in the 40th degree of the thermom.

water will not diſſolve $\frac{1}{12}$ of its weight of alum, borax, vitriolated tartar, corroſive ſublimate and a great many other ſalts; however, as ſuch a table cannot fail of being uſeful in chemical, and perhaps medical reſearches, it would be worth while to make it more general, either by diſſolving a leſs portion of ſalt, or making uſe of a greater degree of heat.

Experiment XI.

To theſe tables I have ſubjoined another of a different nature, wherein the ſpecific gravities of water impregnated with different quantities of the ſame ſalt from $\frac{1}{3}$ down to the 1024th part of the weight of the water, are determined. I cannot accuſe myſelf of careleſſneſs in making any of the experiments from which the table is formed; but part of it being made in a room where the heat was about 55°, and the other in my laboratory, when it did not exceed 46°, a certain inaccuracy, though it will be a very ſmall one and ſcarce ſenſible in the weight of the ſmall body which I uſed, will attend it upon that account. The ſalt was ſea ſalt of the fineſt kind, and extremely dry; many of the experiments were repeated.

A Table of the ſpecific gravity of water impregnated with different quantities of ſea ſalt. Thermometer between 46 and 55°.

Water	1,000
Salt $\frac{1}{3}$	1,206

$\frac{1}{4}$

$\frac{1}{4}$	1,160
$\frac{1}{5}$	1,121
$\frac{1}{6}$	1,107
$\frac{1}{7}$	1,096
$\frac{1}{8}$	1,087
$\frac{1}{9}$	1,074
$\frac{1}{12}$	1,059
$\frac{1}{14}$	1,050
$\frac{1}{15}$	1,048
$\frac{1}{16}$	1,045
$\frac{1}{18}$	1,040
$\frac{1}{21}$	1,032
$\frac{1}{24}$	1,029
$\frac{1}{27}$	1,027
$\frac{1}{28}$	1,025
$\frac{1}{30}$	1,024
$\frac{1}{32}$	1,023
$\frac{1}{36}$	1,020
$\frac{1}{39}$	1,019
$\frac{1}{42}$	1,015
$\frac{1}{48}$	1,014
$\frac{1}{54}$	,013
$\frac{1}{56}$	1,012
$\frac{1}{72}$	1,009
$\frac{1}{84}$	1,007
$\frac{1}{108}$	1,006
$\frac{1}{126}$	1,005
$\frac{1}{144}$	1,004
$\frac{1}{162}$	1,003
$\frac{1}{192}$	1,0029
$\frac{1}{256}$	1,0023
$\frac{1}{320}$	1,0018

$\frac{1}{448}$	1,0017
$\frac{1}{512}$	1,0014
$\frac{1}{848}$	1,0008
$\frac{1}{1024}$	1,0006

From this table it will be eaſy to determine how much the ſpecific gravity of water is increaſed by the ſolution of a given quantity of ſalt, and, *vice verſa*, if we know the ſpecific gravity of any ſolution of ſalt, we may form a good conjecture of the quantity of ſalt contained in it, which obſervation may be of ready uſe in eſtimating the ſtrength of brine ſprings, and of ſea water, taken up in different climates, or upon different coaſts in the ſame climate. Thus, if a ſalt ſpring, or ſea water, ſhould weigh $\frac{1}{56}$ more, bulk for bulk, than common water; we may conclude that it contains $\frac{1}{36}$ of its weight of ſalt; if $\frac{1}{40}$, it hath nearly $\frac{1}{28}$; if $\frac{1}{25}$, $\frac{1}{18}$; if $\frac{1}{20}$, $\frac{1}{14}$; and ſo on: we may always find limits near enough to form a concluſion from, though the exact number denoting the weight in any particular caſe ſhould not be met with in the table.

After I had drawn up the preceding account of the experiments which I had made, I received the Berlin Memoirs for 1762, publiſhed laſt year, in which there is a memoire entitled—*Experiences ſur le poids du ſel et la gravité ſpécifique des ſaumures faites et analyſées, par M. Lambert.* In this memoire, the very ingenious author hath made much uſe of the principle, which I have endeavoured to call in queſtion in the beginning of this paper; and hath calculated the different quantities of ſea ſalt, which are abſorbed into the pores of water, when a given quantity is diſſolved in different quantities of water. The admiſſion of this principle hath drawn him into ſome concluſions which ſeem not quite conſonant to true philoſophy; as when he aſſerts that the quantity which is abſorbed into the pores, is not proportional to the number of the pores or the quantity of water: for, if a given quantity of water, ſuppoſe *A*, will abſorb a given quantity of any ſalt, ſuppoſe *a*, I can ſee no poſſible reaſon why *mA* ſhould not abſorb *ma*: for imagining *mA* to be divided into portions reſpectively equal to *A*, and equal quantities of ſalt to be diſſolved in each of them; then, from the ſuppoſition, each of them will abſorb *a*; and when they are all mixed together, as no precipitation will enſue, the ſum, or *mA*, muſt have abſorbed *ma*. But I have no inclination to animadvert upon what ſeems to be a ſmall miſtake of an author, whoſe various writings do much honour to philoſophy in general, nor to involve myſelf in a diſpute with any one. The following experiment may perhaps be thought concluſive againſt the doctrine of ſalts being abſorbed into the pores of water: I took a large glaſs receiver, containing near ſix gallons; into its neck, by means of a hole bored through a cork, I cemented a ſmall glaſs tube; and having filled the whole up to the middle of the tube with water, I dropped in a piece of ſea ſalt, weighing leſs than one forty thouſandth part the weight of the water: the water inſtantly roſe in the tube, continued ſinking during the ſolution, but at laſt remained as much elevated as it would have been had there been no more water than what would have been ſufficient to diſſolve it. In making this experiment, the receiver ſhould not be touched by the hand, for its parts ſuddenly expanding themſelves occaſion an inſtantaneous ſinking of the water in the tube, as I have frequently experienced, and might thus induce a ſuſpicion of the water's not being elevated by the addition of ſalt. I would not be underſtood from theſe experiments to deny the poroſity of water, ſince philoſophers have thought that the paſſage of light through it, and other phænomena indicate the exiſtence of vacuities in it; but I cannot believe, however ſolution be carried on, that the ſmalleſt quantity of ſalt can be diſſolved in the largeſt quantity of water, without increaſing its magnitude. The cauſe of the water's ſinking during ſolution doth not appear to be ſo certain; the eſcape of air, to which all the appearances induced me to refer it, and to which it may perhaps ſtill be owing, ſeems to be liable to ſome objections, not only from the experiments I have before mentioned, but from the following.

EXPERIMENT XII.

I took two matraſſes of equal dimenſions, one filled with common water, the other with boiled water. I poured into them equal quantities of oil of vitriol; in the firſt there ſeemed to be an univerſal precipitation of air, as it were, from every particle of the fluid, which, by little and little, formed itſelf into larger bubbles, and aſcending through the neck, eſcaped; in the other, hardly any air could be obſerved, the water ſunk during the ſolution of the acid very apparently, yet $\frac{1}{1000}$th part of the water's weight of acid cauſed a ſenſible elevation: ſo that, whatever may be thought of the cauſe of the water's ſinking during the ſolution of a ſalt, the principle of its being to a certain degree imbibed into the pores of water ſeems in no caſe to be true, whether the ſalt be in a concrete or fluid form. This ſubject may receive ſome illuſtration from what is obſerved in the freezing of water; ice from common water is always ſpecifically lighter than water, from its retaining in its concrete form ſeveral air-bubbles, which enlarge its bulk without adding to its weight; this ice, when put into a matraſs, after the manner in which all the preceding experiments with ſalts were made, would elevate the water moſt upon the firſt immerſion: the water would ſink as the ice melted; equal portions of ice would produce equal elevations both before and after ſolution; the air would be ſeparated in a form more or leſs viſible, according to the circumſtances in which the experiment ſhould be tried; and not the ſmalleſt portion of ice could be diſſolved without increaſing the bulk of the whole. Salts do not ſeem to differ much from ice in the manner of their formation, and as ſimilar phænomena attend their ſolution in water, why may we not explain them from the ſame cauſe? But if any one ſhould think differently, notwithſtanding the experiments which have been produced, I profeſs myſelf extremely ready to liſten to any reaſoning founded upon experiment which may tend to prove my opinion to be erroneous; having no partiality for any thing but truth, nor being aſhamed of ignorance or miſtake in any matter, reſpecting the comprehenſion or explication of even the minuteſt operation of nature: *ego quidem hoc ſum contentus, quod licet quo quidque fiat ignorem, quid fiat intelligo.*

XXIX. *An Account of an Occultation of the Star ζ Tauri by the Moon, obſerved at* Leiceſter*: By the Rev. Mr.* Ludlam, *in a Letter to the Rev. Mr.* Maſkelyne, *Aſtronomer Royal.*

Reverend Sir,

Read June 4, 1770.

I Here ſend you an account of the occultation of the ſtar ζ Tauri by the moon, as we ſaw it at Leiceſter April 28, 1770. The neceſſary obſervations for examining the clock, are ſubjoined, which I think ſhould always be done by thoſe who have not fixed obſervatories; or where their inſtruments are not well adjuſted at firſt, and frequently examined afterwards. Without being thus particular, no one can judge what degree of credit may be afforded to ſuch obſervations; and it has, beſides, this advantage, that if a miſtake ſhould be made in the calculations grounded on them, any perſon verſed in Aſtronomy can rectify it.

I beg the favour of you to communicate this to the Royal Society; and am,

With the greateſt reſpect,

Their and your moſt humble ſervant,

Leiceſter, April 29, 1770.

W. Ludlam.

The immerſion was noted at $\mathrm{IX}^{h}\ 41^{m}\ 7^{s}$ by the clock. I think it might be two ſeconds ſooner, becauſe the clock being of neceſſity at a diſtance from the teleſcope, the inſtant of the immerſion was ſignified by ſtriking upon a bell. The emerſion was about $\mathrm{X}^{h}\ 31'$ but with ſome uncertainty, the ſtar being hid by a cloud at its firſt coming out.

By the obſerved tranſits of the ſun and ſtars, the clock loſt three ſeconds between the 25th and 28th. On the 25th, by correſponding altitudes, the clock was 1′ 46″, 8 too ſlow; whence on the 28th it was 1′ 49″, 8 too ſlow. This confirms the obſervation made by correſponding altitudes on the 28th, by which it was 1′ 50″ too ſlow at noon: the clock was then loſing at the rate of four ſeconds a day; whence, on the 28th at IX^{h}, it was 1′ 51″, 5 ſlower than mean time. The equation of time on the 28th at IX^{h} was 2′ 47″, 5, whence the immerſion was at $\mathrm{IX}^{h}\ 45'\ 44''$ apparent time.

The Teleſcope made uſe of was one of Dollonds, with a triple object glaſs of $33\frac{1}{3}$ inches focal diſtance, and which magnifies 52 times.

Tranſits

Day of the month 1770	Tranſits Time by the clock Paſſage over Firſt wire		Meridian			Third wire		Object
	M	S	H	M	S	M	S	
April 24	53	54	XXIII	54	42	55	27½	Sun
	56	6		56	53½	57	58	
26	7	36	X	8	21	9	5½	γ Virginis
27	11	8	IX	11	55	12	41	β Leonis
	3	39	X	4	25	5	9½	γ Virginis
	17	28		18	24	19	9	δ Virginis
	46	35		47	21	48	6½	Spica V
	53	24	XXIII	54	11	54	56¾	Sun
	55	35		56	22	57	8	
28	7	9	IX	7	56	8	43	β Leonis
	59	41	X	0	26½	1	11	γ Virginis
	13	30		14	25½	15	10	δ Virginis
	42	36½		43	23	44	8	Spica V
	53	10	XXIII	53	57½	54	44	Sun
	55	21½		56	9	56	55	
29	3	10½	IX	3	58	4	44	β Leonis
	53	0	XXIII	53	47	54	33½	Sun
	55	13		56	0	56	46	

Correſponding altitudes taken with an Hadley's quadrant of ſeven inches radius, by reflection from water.

	Eaſtern Az.	Weſtern Az.	Meridian
	Sun April 25, 1770		
	Double Alt. 65° Index err. + 1^{m}		
	Time by the clock.		
Up. limb	VIII 21 48½	III 30 43	XI 56 15¾
Center	23 42	28 52	17
Low. limb	25 37	26 54½	15¾
	Second ſet. Doub. Alt. 67° 30′		
Up. limb	VIII 30 45	III 21 45	15
Center	32 40	19 49	14½
Low. limb	34 35½	17 53	14¼
	Third ſet. Doub. Alt. 70°		
Up. limb	VIII 39 53	III 12 34½	13¾
Center	41 50	10 41	15½
Low. limb	43 48		
	Mean of all three ſets		XI 56 15,2
	Cor. for ½ interval 3^{h} 20′		— 16,3
	Paſſage over true meridian		XI 55 58,9
	Clock too ſlow		1 46,8
	Sun April 28, 1770		
	Double Alt. 79° 11′ Index err. + 1^{m}		
	Time by the clock.		
Up. limb	IX 8 12½	II 43 6½	XI 55 39½
Center	10 16	41 3	39½
Low. limb	12 22½	38 56	38¾
	Mean		XI 55 39,5
	Cor. for ½ interval 2^{h} 20′		— 13,5
	Paſſage over true meridian		XI 55 26
	Clock too ſlow		1 50

XXX. *Extract*

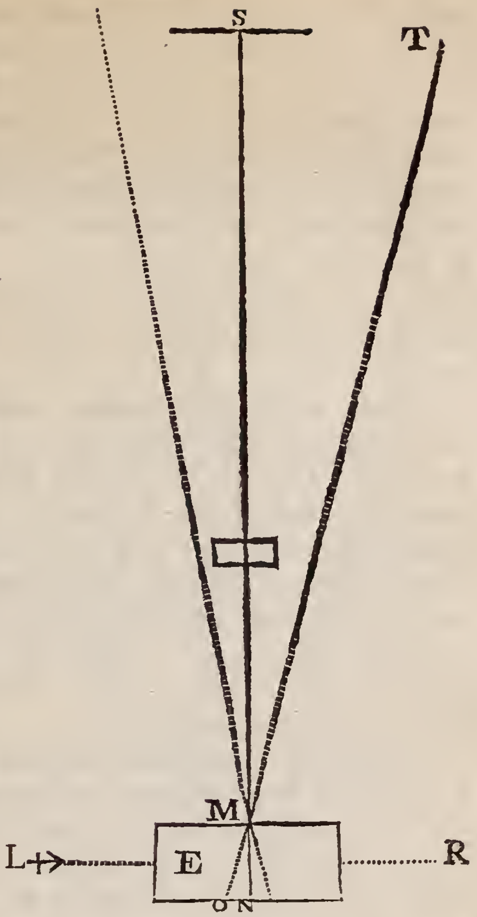
Received February 8, 1770.

XXX. *Extract of a Letter from* John Winthrop, *Eſq; F. R. S. Holliſian Profeſſor of Mathematics and Natural Philoſophy, at* Cambridge, N. England; *to* B. Franklin, *LL. D. F. R. S. Dated Sept.* 6. 1769.

Read April 5, 1770.

I FIND that Mr. Bliſs and Mr. Hornſby, in their calculations in the Philoſophical Tranſactions, ſuppoſe the phaſes of the tranſit of Venus, to be accelerated by the equation for the aberration of light, which amounts to 55″ of time. According to my idea of aberration, I ſhould think the tranſit would be retarded by it. I can very eaſily ſuppoſe that I am in an error; and that I may more readily be led out of it, I beg leave to lay before you the ſeveral ſteps by which I have been led into it. And I think it will be beſt to take ſome ſimilar inſtance, rather than to conſider the thing in a general abſtract manner.

1. Let



1. Let the parallelogram E repreſent a veſſel ſailing in the line LR, from left hand to right; and S, a fixed ſtation, *e. g.* a caſtle, diſcharging balls in the right line SM, perpendicular to the route of the veſſel. If the veſſel had been at reſt, a ball arriving at the middle of it, M, would have gone right acroſs it, to N. But as it is ſuppoſed to be ſailing, the ball will not go right over from M to N, but will croſs the deck obliquely, in another right line, as MO, and ſo will be left behind towards the ſtern as much as the veſſel had gone forward, while the ball was croſſing it; and MN will be to NO as the velocity of the ball to the velocity of the veſſel. Thus, to the people on board, the ball would ſeem to move obliquely acroſs the deck, as if it came from ſome

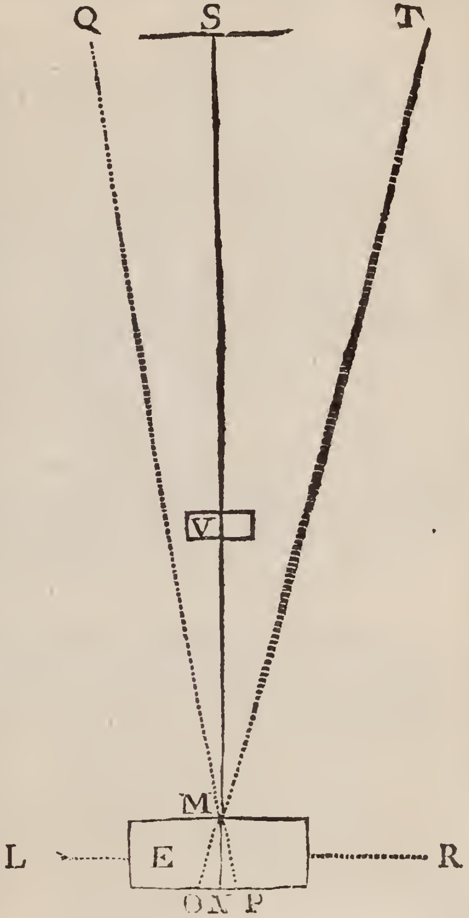
point T in the line OM, produced, inſtead of coming from S. And a tube capable of receiving the ball, would allow the ball to paſs through it without ſtriking its ſides, if it were inclined forward in the direction OM; which it would not do in any other ſituation. The angle OMN or SMT anſwers to the aberration; and ſuppoſing S to be the ſun, and E, the earth, this angle is 20″; and the general effect is, to make the ſun, or any fixed ſtar, to appear farther that way towards which the earth is moving.

2. Let us ſuppoſe another veſſel V, between S and E, ſailing the ſame way as E, in a parallel direction. If both the veſſels ſailed with the ſame velocity, a ball from V coming to M, would go right acroſs to N, juſt as if both of them had been at reſt; becauſe the ball, while croſſing the veſſel E, would be carried juſt as far to the right hand as the points M and N are. And a tube to receive it muſt be held in the direction MN. So here would be no aberration of the veſſel V.

3. Suppoſe V to move the ſame way, but ſlower. A ball from V would now be really carried forward, that is, to the right hand, though not ſo far as in the ſecond ſuppoſition; and therefore would be left behind in reſpect of the veſſel E; and ſo, would come to the ſide of the veſſel ſomewhere between O and N; but the greater its velocity towards the right, the nearer to N. So that if the velocity of V were to be continually increaſing from nothing till it became equal to that of E, a tube to receive the ball muſt be held firſt in the direction OM, looking forward, and afterwards, more and more in-

clined till it came into the perpendicular direction MN. From hence it is natural to conclude,

4. That if V move the ſame way, but ſwifter, a tube to receive the ball muſt be reclined backward. For the ball would now be carried to the right hand farther than in the ſecond ſuppoſition; and therefore would come to the other ſide of the veſſel at ſome point P on the right hand of N, as if it proceeded from ſome point Q on the left hand of S.



This laſt ſeems to be the caſe of the tranſit, by ſuppoſing S to be the ſun, E the earth, and V the planet Venus paſſing between them from left to right, and with a greater velocity than the earth (greater, nearly as 24:20). And it ſhould ſeem that the aberration muſt make Venus appear farther to the left hand, or to the Eaſt from the ſun, and conſequently retard the tranſit, and make it happen later than it would otherwiſe do.

Thus, Sir, I have explained very particularly my apprehenſion of the matter; and I make no doubt you will immediately diſcover where the error lies, and ſhall take it as a great favour if you will pleaſe to point it out to me.

XXXI. *Extract of a Letter from Mr.* Mallet, *of* Geneva, *to Dr.* Bevis, *F.R.S.*

Read June 21, 1770.

I HAD the pleaſure of writing you a few lines in the month of Auguſt, laſt year, when I ſent you my obſervations relative to the tranſit of Venus, which the Peterſburg Academy has printed without my knowledge, whilſt I was yet in Lapland. I left Ruſſia ſoon afterwards, and have been five or ſix months in my own country. Part of this time I have employed in reducing and computing my obſervations made in the North, to get what uſeful reſults I could from them, which I have juſt now ſent to Peterſburg, to be printed in the Commentaries of the Academy. As it may be ſome time before that volume will be publiſhed, I thought, Sir, you might be willing to be informed of ſome of the principal conſequences reſulting from my obſervations.

1°. To determine the latitude of Ponoi, where I obſerved the tranſit, a great number of meridian altitudes of the ſtars and Sun, taken with a quadrant of 2 feet radius, made at London by Mr. Siſſon, gave the elevation of the pole 67° 04′ 30″. I was not able to make any other obſervation but that of the Sun's eclipſe, on the 4th of June, for determining the longitude. I obſerved with a 12 feet achromatic teleſcope of Dollond, the end at $0^h\ 07'\ 55''$ apparent time. The celebrated M. L. Euler, who has computed ſeveral obſervations of this eclipſe made in different places, finds, by my obſervation, the difference of meridians between Paris and Ponoi $2^h\ 25'\ 33''$, that is, 38° 51′ Eaſt of Paris.

2°. On the obſervation of the tranſit of Venus.

I obſerved, with the ſame teleſcope, the interior contact at the entry at $10^h\ 15'\ 4''$ apparent time. I have computed very ſcrupulouſly the effect of parallax on the moment of this contact; I made uſe of the ſame elements that M. de la Lande gives in his Memoire, printed in 1764, excepting that I aſſume the neareſt diſtance of the centres of Venus and the Sun, ſeen from the centre of the Earth 10′ 27″; which quantity I deduced from the whole duration, obſerved at Hudſon's Bay, by Meſſ. Dymond and Wales, as given in the news-papers. I find the effect of parallax 7′ 03″ of time, whereby I muſt have ſeen the contact ſooner than at the earth's centre. The computation of my obſervation gives moreover the moment of the conjunction at $12^h\ 46'\ 21\frac{1}{2}''$ apparent time at Ponoi, and the geocentric latitude of Venus for that moment 10′ 33″,9.

If the neareſt diſtance of the centres be taken 5″ leſs, I find the effect of parallax 7′ 11″ of time, that is, 8″ greater, the latitude becomes 5″ leſs, and the moment of the conjunction 1′ 28″ later.

3°. I have made a great number of obſervations for determining the force of gravity and the length of the ſimple pendulum ſwinging ſeconds. I uſed an invariable pendulum which M. de la Condamine got conſtructed at Quito, when the French academicians went thither to meaſure a degree of the meridian, which he was pleaſed to ſend me to Peterſburg; this pendulum, which is no other than a ſimple ſteel rod fixed to a lentille, made at Para 98740 oſcillations in 24 hours of mean time, and at Paris 98891 in the ſame time. I made experiments with this ſame pendulum at Peterſburg, before my departure for Lapland, and have repeated them ſince my return thither. They give the number of oſcillations in 24 hours of mean time 98941, having been careful to preſerve conſtantly the ſame temperature, and to cauſe the pendulum to ſwing very ſmall arcs. At Ponoi, I found the number of oſcillations 98946. Hence it follows, that the ſimple pendulum, which beats ſeconds at Peterſburg, will be 441,02 lines (Paris meaſure), that is, $\frac{45}{100}$ lin. longer than the pendulum which beats ſeconds at Paris; and the pendulum at Ponoi will be 441,22 lin. that is $\frac{65}{100}$ lin. longer than that of Paris.

The exceſs of the Paris pendulum above that at the equator has been determined by the academicians 1,50 lin.; and admitting Sir Iſaac Newton's principle, and Huyhens', that the increaſe of gravity, in approaching the pole, follows the ratio of the ſquare of the ſine of latitude, we ſhould find 1,98 lin. for the exceſs of the Peterſburg pendulum above that at the equator, inſtead of 1,95, which I find by my experiments; the ſame calculus would give 2,24 lin. for the exceſs of the Ponoi pendulum, inſtead of 2,15 lin. which reſults from my experiments. Hence it would follow that the increments of gravity follow a ratio ſomewhat greater than that of the ſquares of the ſines of latitude; and this reſult is confirmed by the experiments made at Pello in Lapland, by the French academicians.

4°. I obſerved ſeveral times very exactly at Ponoi, the declination of the magnetic needle 1° 10′ Eaſt.

5°. Exact obſervations of the inclination of the needle made in different places of our globe, combined with thoſe made long ago on the declination, would be very intereſting and proper for the advancement of our knowledge, as to the theory of magnetiſm, which hitherto is but little underſtood. It is the difficulty of making ſuch obſervations, and obtaining accurate reſults, which has diſcouraged phyſicians and travellers; but it is ſurprizing that ſo little has been done in this matter, ſince Dr. Daniel Bernoulli furniſhed us with new ideas for conſtructing a machine fit for determining the true magnetic inclination, in a Memoire, which gained the prize propoſed for this ſubject by the Paris Academy, in 1743. He got an inclinatory needle conſtructed at Baſle, upon new principles, and the experiments he made aſſured him of ſucceſs; he found the inclination at Baſle 71½ degrees.

Mr. Euler, the ſon, made uſe of the ſame compaſs at Berlin, but by employing a method entirely different from that of Dr. Bernoulli. He gives the particulars thereof in the Memoirs of the Academy of Berlin, 1755. After a great number of obſervations, he found the inclination to be then at Berlin between 72½ and 73 degrees. At Peterſburg I got conſtructed a like machine, and uſed it for determining the inclination at Peterſburg, Kola, and Ponoi; I employed both the methods of Meſſ. Bernoulli and Euler, and found a wonderful agreement in the reſults drawn from a great number of experiments. Two needles made by different artiſts, one at Baſle, the other at Peterſburg, conſequently ſuſceptible of a different magnetic force, produced but very minute differences, inevitable in ſo delicate experiments; the ſeveral particulars are recited in the papers I have ſent to the Peterſburg Academy, from whence it may be concluded that it is poſſible to determine with this inſtrument the true inclination of the magnetic needle, without being any way liable to an error of half a degree in the reſult, which in my experiments is as follows.

At Peterſburg lat. 59° 55′ longitude 48° the inclination was in 1769 73°$\frac{2}{3}$
At Ponoi 67 04 58° 51′ 76 $\frac{1}{2}$
At Kola 68 54 49 45 77 $\frac{3}{4}$

6°. I have moreover ſubjoined the ſeveral particulars of my meteorological obſervations, during my five months ſtay in Lapland. Let it ſuffice to give you the mean height of the barometer, which I found 27 in. 6,2 lin. (Paris) in March; 27 in. 5,5 lin. in April; 27 in. 7,6 lin. in May; and 27 in. 5,8 lin. in June: the mean height for four months being 27 in. 6$\frac{1}{3}$ lin. I could not well meaſure exactly my elevation above the level of the ſea, but I take it not to exceed 40 or 50 toiſes.

If you think, Sir, this ſummary worthy the notice of the Royal Society, be ſo good as to lay it before them.

I am, dear Sir,

Your affectionate humble ſervant,

Cour de St. Pierre,
Geneve, 13 Avril, 1770.

J. A. Mallet.

Received

Received May 17, 1770.

XXXII. *Experiments on the Blood, with ſome Remarks on its Morbid Appearances;* *by* William Hewſon, *F. R. S.*

Read June 14 & 21. 1770.

AS the following Experiments are made on a ſubject generally thought important, and as the inferences which I have ventured to draw from them ſeem to explain ſome appearances in diſeaſes, they will not, I flatter my ſelf, be thought altogether unworthy the attention of this learned Society.

When freſh blood is received into a baſon, and ſuffered to reſt, in a few minutes it jellies, or coagulates, and ſoon after ſeparates into two parts, diſtinguiſhed by the names of *craſſamentum* and *ſerum*. Theſe two parts differ in their proportions in different conſtitutions: in a ſtrong perſon, the *craſſamentum* is in greater proportion to the *ſerum* than in a weak one; and the ſame difference is found to take place in diſeaſes; thence is deduced the general concluſion, that the leſs the quantity of *ſerum* is in proportion to the *craſſamentum*, bleeding, diluting liquors, and a low diet, are the more neceſſary: whilſt in ſome dropſies and other diſeaſes where the *ſerum* is in a great, and the *craſſamentum* in a ſmall proportion, bleeding and diluting would be highly improper. As it is therefore ſuppoſed uſeful to attend to the proportions of theſe parts in many diſorders, and even to take indications of cure from them, it has been an object with thoſe who have made experiments on the blood, to determine the circumſtances on which its more perfect ſeparation into theſe two parts depends; it being obvious, that till this be done, our inferences from their proportions will be liable to conſiderable fallacies. Two of the lateſt writers on this ſubject agree, that if the blood, after being taken from a vein, be ſet in a cold place, it will not eaſily ſeparate, and that a moderate warmth is neceſſary: this is a fact that is evinced by daily experience. They likewiſe ſay, that the heat ſhould be leſs than that of the animal, or than 98° of Fahrenheit's thermometer; and that if freſh blood be received into a cup, and that cup put into water heated to 98°, it will not ſeparate; nay, they even ſay, that it will not coagulate; but this, I am perſuaded from experiments, is ill-founded.

EXPERIMENT I.

A tin-veſſel containing water, was placed upon a lamp which kept the water in a heat that varied between 100 and 105 degrees. In this water was placed a phial, containing blood that inſtant taken from the arm of a perſon in health; the phial was previouſly warmed, then filled, and corked to exclude air. In the ſame water was placed a tea-cup half full of blood, juſt taken from the ſame perſon;

a third portion of the blood was then received from the ſame vein into a baſon, and was ſet upon a table, the heat of the atmoſphere being at 67°. Now, according to their opinion, the two former ſhould neither have coagulated nor ſeparated, when that in the baſon began to ſeparate; but, on the contrary, they were all three found to coagulate nearly in the ſame time; and thoſe in the warm water, not only did ſeparate as well as the other, but even ſooner.

EXPERIMENT II.

The ſame experiment was repeated on the blood of a perſon that laboured under the acute rheumatiſm, whilſt the heat of the atmoſphere was no higher than 55°, and that of the warm water was 108°; and the reſult of this experiment was not only a confirmation of what was obſerved in the firſt, but it even ſhewed, that, that degree of heat was ſo far from leſſening, that it increaſed the diſpoſition to coagulate; for the blood in the cup and in the phial was not only coagulated, but the ſeparation was much advanced before the whole of the blood in the baſon was coagulated. Thence I am led to conclude, that the ſeparation of the blood in a given time, is in proportion as the heat in which it ſtands is nearer to the animal heat, or 98°; or greater in that heat than in any of a leſs degree. And I am confirmed in this inference by experiments hereafter to be related, where the blood in the living animal whilſt at reſt was found both to coagulate and to ſeparate.

It is well known, that the *craſſamentum* conſiſts of two parts, of which one gives it ſolidity, and is by ſome called the fibrous part of the blood, or the gluten, but by others with more propriety termed the *coagulable lymph*; and of another, which gives the red colour to the blood, and is called the red globules. Theſe two parts can be ſeparated by waſhing the *craſſamentum* in water, the red particles diſſolving in the water, whilſt the coagulable lymph remains ſolid. That it is the coagulable lymph which by its becoming ſolid gives firmneſs to the *craſſamentum*, is proved by agitating freſh blood with a ſtick, ſo as to collect this coagulable lymph on the ſtick, in which caſe the reſt of the blood remains fluid *.

The ſurface of the *craſſamentum*, when not covered with a cruſt, is in general of a more florid red than the blood was when firſt taken from the vein, whilſt its bottom is of a dark colour, or blackiſh. This floridneſs of the ſurface is juſtly attributed by ſome of the more accurate obſervers to the air, with which it is in contact; for, if the *craſſamentum* be inverted, the colours are changed, at leaſt that which is now become the upper ſurface aſſumes a more florid redneſs. This difference of colour, others have endeavoured to explain from the different proportions of

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* It may be proper to mention here, that till of late the coagulable lymph has been confounded with the ſerum of the blood, which contains a ſubſtance that is likewiſe coagulable. But in theſe papers, by the *lymph*, is always meant that part of the blood which jellies, or becomes ſolid ſpontaneouſly when blood is received into a baſon, which the coagulable matter that is diſſolved in the ſerum does not; but agrees more with the white of an egg, in remaining fluid when expoſed to the air, and coagulating when expoſed to heat, or when mixed with ardent ſpirits, or ſome other chemical ſubſtances.

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the red particles, or globules as they are called, which, ſay they, being in greater proportion at the bottom of the *Craſſamentum*, makes it appear black; but, if inverted, the globules then ſettle from the ſurface which is now uppermoſt, and that becomes redder. But this I think is not probable; for the lymph in the *craſſamentum* is ſo firmly coagulated, as to make it too denſe, to allow of bodies even heavier than the red particles to gravitate through it; for example gold. That air has the power of changing the colour of the blood, has been long known; and the following experiment ſhews it very ſatisfactorily, and hardly leaves room to refer the appearance to another cauſe.

EXPERIMENT III.

Having laid bare the jugular vein of a living rabbit, I tied it up in three places; then opening it between two of the ligatures, I let out the blood, and filled this part of the vein with air. After letting it reſt a little till the air ſhould become warm, I took off the ligature, which ſeparated the air from the blood, and then gently mixed them, and I obſerved that the venous blood aſſumed a more florid redneſs, where in contact with the air-bubbles, whilſt at other parts it remained of its natural colour.

There is a difference between the arterial and venous blood in colour; the former is of a florid red like the ſurface of the *Craſſamentum*, the latter is dark or blackiſh like the bottom of the craſſamentum. This change in its colour is produced on the blood as it paſſes through the lungs, as we ſee by opening of living animals *; and as a ſimilar change is produced by air applied to blood out of the body, it is preſumed that the air in the lungs is the immediate cauſe of this change; but how it effects it, is not yet determined.

As the blood is changed to a more florid red in paſſing through the lungs, or from the venous to the arterial ſyſtem, ſo it loſes that colour again in paſſing from the arteries to the veins in the extreme parts, eſpecially when the perſon is in health; but every now and then we obſerve the blood in the veins more florid than is uſual, and it likewiſe frequently happens in blood-letting, that the blood which comes firſt out is blackiſh, but afterwards it becomes more florid: in theſe caſes, the arterial blood paſſes into the veins without undergoing that change which is natural to it.

Some of the neutral ſalts have a ſimilar effect on the colour of the blood to what air has, particularly nitre; thence ſome have attributed the difference of colour in the arterial and venous blood to nitre, which they ſuppoſed was abſorbed from the air whilſt in the lungs. But we know that this is a mere ſuppoſition, for air contains no nitre. Indeed nitre is far from

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* That this change is really produced in the lungs, I am perſuaded from experiments, in which I have diſtinctly ſeen the blood of a more florid red in the left auricle, than it was in the right. But ſome authors of the greateſt authority ſay, that they could not obſerve any ſuch difference in a great number of experiments which they made; but this I ſhould attribute to their having been later in opening the left auricle after the collapſing of the lungs than I was; for it ſeems probable, that whatever is the alteration produced on the blood in its circulation through this organ, after it is collapſed, this change cannot take place.

being the only neutral ſalt which has this effect on the blood, almoſt all the neutral ſalts have the ſame. In making ſome experiments on this ſubject, I have obſerved a much more remarkable effect which neutral ſalts have upon the blood; and that is, being mixed with it when juſt received from the vein, they prevent its coagulation, or keep it fluid, and yet, upon adding water to the mixture, it then jellies or coagulates: thus, if ſix ounces of human blood be received from a vein upon half an ounce of Glauber's ſalt reduced to a powder, and the mixture agitated ſo as to make the ſalt be diſſolved, that blood will not coagulate on being expoſed to the air, as it would have done without the ſalt; and if to this mixture about twice its quantity of water be added, in a few minutes the whole will be jellied or coagulated, and on ſhaking the jelly, the *coagulum* will be broken, and the part ſo coagulated can be now ſeparated as it falls to the bottom, and proves to be the lymph.

In theſe mixtures of the blood with neutral ſalts, the red particles readily ſubſide (eſpecially if human blood be uſed) and the ſurface of the mixture becomes clear and colourleſs; and being poured off from the red part, it is found to contain the coagulable lymph, which can be ſeparated by the addition of water.

I have tried all the neutral ſalts, and have made a table of their effects on the blood, but this table I ſhall not trouble the Society with at preſent: it may be ſufficient to obſerve that in general they agree in producing this change *. And it is leſs neceſſary to be particular in giving a detail of their effects, from our not knowing of any uſe this would be in medicine, becauſe we muſt not conclude that their effects in the body would be the ſame as we ſee they are out the body. Indeed, theſe experiments, as well as many others, were not made ſo much with a view of any immediate application to medicine, as to determine the properties of the blood chemically: for, having ſet out with a perſuaſion, that a more particular acquaintance with the properties of this fluid was neceſſary before we could arrive at the knowledge of ſome of the animal functions, ſuch as the manner in which the bile and other ſecreted fluids are formed, I therefore determined to do my utmoſt to throw ſome light on this ſubject. It was with this view that I have made ſome experiments even on living animals, being convinced that ſuch experiments could not otherwiſe be made ſatisfactorily.

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* It may be neceſſary to obſerve here, that thoſe made with the volatile alkali, and with the earth of allum, are to be excepted.

When blood is thus kept fluid by neutral ſalts, it ſtill retains its property of being coagulable by heat, and by other ſubſtances as before, air excepted. This method of keeping the blood fluid may therefore be uſeful, by giving us an opportunity of making ſome experiments on the blood, which we could not otherwiſe do, from its coagulating ſo ſoon when taken from the blood-veſſels.

This property of one of the neutral ſalts has been long known, amongſt thoſe who prepare blood for food; for it has long been a practice with ſuch people, to receive blood into a veſſel containing common ſalt, and to agitate it as faſt as it falls, by which means the coagulation is prevented, and the blood remains ſo fluid as to paſs through a cloth, without leaving any *coagulum* behind: by this means they have an opportunity of mixing it with other ſubſtances for the uſes of the kitchen.

Although the coagulable lymph ſo readily becomes ſolid when expoſed to the air, yet whilſt it circulates it is far from being ſolid: it has indeed been ſuppoſed to be fibrous, even whilſt moving in the blood-veſſels, but erroneouſly.

It is this coagulable lymph which forms the inflammatory cruſt, or *buff* as it is called. It likewiſe forms *polypi* of the heart, and ſometimes fills up the cavities of aneuriſms, and plugs up the extremities of divided arteries. It is ſuppoſed, by its becoming ſolid in the body, to occaſion obſtructions and inflammations; and even mortifications, from the expoſition to cold, have been attributed to its coagulation. In a word, this lymph is ſuppoſed to have ſo great a ſhare in the cauſe of ſeveral diſeaſes, that it would be deſireable to aſcertain what brings on that coagulation, either in the body or out of it.

The blood, when received into a baſon and ſuffered to reſt in the common heat of the atmoſphere, very ſoon jellies or coagulates; the part which now becomes ſolid is the coagulable lymph, as has been ſhewn above. The circumſtances in which it now differs from what it was in the veins, are theſe: it is laid open to the air, to cold, and is at reſt; for whilſt in the body, air is excluded, it is always of a conſiderable warmth, and is always in motion. The queſtion is, to which of theſe circumſtances its coagulation whilſt in the baſon is chiefly owing. This queſtion, I believe, cannot well be anſwered from the experiments that have hitherto been made. It has indeed been ſaid, that the cold alone coagulated it; for, ſay they, if you receive blood into a baſon, and keep that baſon in warm water, and ſtir the blood well, it can be kept fluid. But in the experiments from which this concluſion was made, I find there has been a deception. In ſhort I have found that it coagulates as ſoon when kept warm, and when agitated, as it does when ſuffered to reſt and to cool. As the ſubject ſeemed to me of importance, I have endeavoured to aſcertain the circumſtance to which this coagulation is owing, by ſeveral of experiments, in each of which the blood was generally expoſed to but one of the ſuſpected cauſes at a time. Thus, in order to ſee whether the blood's coagulation out of the body was owing to its being at reſt, I made the following experiment.

EXPERIMENT IV.

Having laid bare the jugular vein of a living dog, I made a ligature upon it in two places, ſo that the blood was at reſt between the ligatures; then covering the vein with the ſkin, to prevent its cooling, I left it in this ſituation. From ſeveral experiments made in this way, I found in general, that after being at reſt for ten minutes, the blood continued fluid; nay, after being at reſt for three hours and a quarter, above two thirds of it was ſtill fluid, though it coagulated afterwards. Now the blood, when taken from a vein of the ſame animal, was completely jellied in about ſeven minutes. The coagulation therefore of the blood in the baſon, and of that which is merely at reſt, are ſo different, that reſt alone can-

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not be ſuppoſed to be the cauſe of the blood's coagulation out of the body.

To ſee the effects of cold on the blood, I made this experiment.

EXPERIMENT V.

I killed a rabbit, , and cut out one of its jugular veins immediately, proper ligatures being previouſly made upon it; I then threw the vein into a ſolution of ſal ammoniac and ſnow, in which the mercury ſtood at the 14th degree of Fahrenheit's thermometer. As ſoon as the blood was frozen I took the vein out again, and put it into luke-warm water till it thawed and became ſoft; I then opened the vein, received the blood into a tea-cup, and obſerved that it was perfectly fluid, and in a few minutes it jellied or coagulated as blood uſually does. Now, as in this experiment the blood was frozen and thawed again without being coagulated, it is evident that the coagulation of the blood out of the body is not ſolely owing to cold, any more than it is to reſt.

Next, to ſee the effects of air upon the blood, I tried as follows.

EXPERIMENT VI.

Having laid bare the jugular vein of a living rabbit, I tied it up in three places, and then opened it between two of the ligatures and emptied that part of its blood. I next blew in warm air into the empty vein, and put another ligature upon it, and letting it reſt till I thought the air had acquired the ſame degree of heat as the blood, I then removed the intermediate ligature, and mixed the air with the blood. The air immediately made the blood florid, where in contact with it, as could be ſeen through the coats of the vein. In a quarter of an hour I opened the vein, and found the blood entirely coagulated: and as the blood could not in this time have been completely coagulated by reſt alone, the air was probably the cauſe of its coagulation.

From comparing theſe experiments, may we not now venture to conclude, that the air is a ſtrong coagulant of the blood, and that it is to this its coagulation when taken from the veins is chiefly owing, and not to cold or to reſt?

It may not be improper to obſerve here, that there are none of the above related experiments I have been obliged to repeat ſo often as the 4th, which was made with a view to determine whether the blood would coagulate by reſt. In the firſt trial which I made, the vein was not opened till the end of three hours and a quarter; and juſt before it was opened I had obſerved through its coats, that the upper part of the blood was tranſparent, owing to the ſeparation of the lymph. On letting out this blood, it ſeemed to me entirely fluid; a part indeed had been loſt, but the greateſt part was collected in the cup, and which afterwards coagulated as blood commonly does when expoſed to the air. From this experiment I imagined that the whole had been fluid; but from others made ſince, I am perſuaded that the part which was loſt had been coagulated; for, from a variety of trials, I now find that though the whole of the blood is not congealed in this time by reſt alone, yet a part of it is. But as it would be treſpaſſing too much on the Society's time to relate every experiment I have been obliged to make for this purpoſe, I ſhall only mention the general reſult of the whole.

After fixing a dog down to a table and tying up his jugular veins, I have in general found, that on opening the veins, at the end of ten minutes, the blood was ſtill entirely fluid, or without any appearance of the coagulation's beginning *. If opened at the end of fifteen minutes, at firſt ſight it appeared quite fluid; but on a careful examination I have found ſometimes one, and ſometimes two or three ſmall particles about the ſize of a pin's head, which are part of the blood coagulated. When opened later than this period, a larger and larger *coagulum* was obſerved; but ſo very ſlowly does this coagulation proceed, that in an experiment where I had the curioſity to compare more exactly the clotted part with the unclotted, I found, after the vein had been tied two hours and a quarter, that the *coagulum* weighed only two grains; whilſt the reſt of the blood, which was fluid, on being ſuffered to congeal, and then weighed, was found to weigh eleven grains. I can advance nothing farther in this part of my ſubject with preciſion. Nor can I pretend exactly to determine the time at which all the blood between the ligatures is coagulated. I have indeed opened ſuch a vein at the end of three days, when I found a thin, white *coagulum*, which was a mere film; the *ſerum* and red particles having diſappeared. But the whole is undoubtedly *congealed* long before this period. The manner in which the blood coagulates, when at reſt in the body, has appeared to me curious, and therefore I have taken the more pains to diſcover how it happens, eſpecially as it may aſſiſt us in judging whether or no it coagulates in the heart, ſo as to form thoſe ſubſtances called *polypi*. The abovementioned times will, I believe, be found to be thoſe at which the blood congeals in the veins of healthy dogs: and as I have found, by experiments, that the blood of a dog and of the human ſubject in health jellies out of the body nearly in the ſame time, that is, it begins in three or four minutes, and is completed in ſeven or eight; I ſhould therefore conclude that the blood coagulates nearly at the ſame period in the veins of the human body. But it may be neceſſary to add here, that from experiments which I have made, I have reaſon to believe that the time at which the blood coagulates, is different in different conſtitutions, and in different diſeaſes. For though the blood of a perſon in health is completely coagulated in ſeven minutes after it is taken out of the veins, yet in ſome diſeaſes, I have found the blood fifteen or twenty minutes, nay even an hour and an half, before it was completely jellied.

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* I ſay, in general, it was fluid at the end of ten minutes; but I muſt likewiſe mention that in one dog I found two very ſmall particles of beginning coagulation, even at this period; yet in another I could not obſerve any ſuch appearance, even at the end of fifteen minutes.

As we ſee in the above related experiments, that the blood coagulates in the body when ſuffered to reſt for a little time, is it not probable that tis to this cauſe its coagulation in thoſe true aneuryſms, which are attended with a pouch, are owing *? For in ſuch enlargements a part of the blood is without motion, which will congeal when at reſt, and in contact with the ſack; and thus one layer may be formed; and the ſack afterwards enlarging, another portion of the blood will then be at reſt; and ſo a ſecond layer may be formed; and thence probably is the origin of thoſe laminated *thrombi* met with in ſuch ſacks.

Likewiſe, to the blood being at reſt, is probably owing its coagulation in the large arteries which are tied after amputation, or other operations; for after moſt of ſuch ligatures there will be a part of the artery impervious, in which the blood can have no motion. The *coagulum* after amputation might indeed be ſuppoſed owing to air; but, conſidering the manner in which arteries are tied whilſt the blood is flowing from them, it does not ſeem probable that the air has any effect on what is above the ligature.

To the blood's being without motion in the cavity of the *uterus*, is its coagulation therein probably owing; hence the origin of thoſe large clots which we ſometimes obſerve to come from this cavity, and which, when they are more condenſed by the ouzing out of the *ſerum*, and of the red particles, aſſume a fleſh-like appearance, and have often been called *moles* or *falſe conceptions*.

In Experiment the 5th, we found that the blood could be frozen and thawed again, without being coagulated: this, likewiſe is an experiment which

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* An inſtance of which may be ſeen in the Medical Obſ. and Inq. vol. i. article xxvii. fig. iii.

I have repeated ſeveral times, that I might be ſure of the fact. I have alſo varied the experiment a little, having ſometimes put the vein into a phial of water, and froze the whole in a ſolution of ſal ammoniac in ſnow; and ſometimes I have put the vein into the ſolution itſelf; and three or four times I have put it into oil, and then froze it; but after all theſe trials, the reſult was found to be the ſame. The blood was always evidently fluid on being thawed, and as evidently jellied when expoſed to the air.

Thus far I have related ſuch experiments as I have made, in order to diſcover the cauſes of the coagulation of the blood, out of the body. Next, if agreeable to this learned Society, I ſhall lay before them ſome other experiments that I have made on this fluid.

Received May 7, 1770.

XXXIII. *On the Degree of Heat which coagulates the Lymph, and the Serum of the Blood; with an Enquiry into the Cauſes of the inflammatory Cruſt, or* Size, *as it is called*: *By* the ſame.

Read Nov. 15. 1770.

IN the preceding paper, beſides mentioning ſome circumſtances which promote the ſeparation of the blood, and which affect its colour, I have enquired into the cauſes to which its coagulation when taken from the veins is owing, and the manner in which it coagulates when at reſt in the body. I ſhall now proceed to lay before this learned Society, an account of ſome other experiments which I have made upon this fluid.

Beſides being coagulated when expoſed to the air, the coagulable lymph, as well as the *ſerum*, is known to be fixed by heat; but the degree of heat has not, I think, been determined. It has been ſuppoſed to require a degree of heat almoſt equal to that which coagulates the *ſerum* *; but a much leſs is neceſſary, as will appear from the following experiments.

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* Vide Traité du Cœur. T. ii. p. 93. Schwenk, Hæmatolog. p. 138.

EXPERIMENT VII.

Having found, from a number of trials, that blood, kept fluid by being mixed with neutral ſalts, had its lymph coagulated by a heat of 125° of Fahrenheit's thermometer, I ſuppoſed that the degree neceſſary for fixing it in its natural ſtate could not be very different from this. I therefore prepared a lamp-furnace with a ſmall veſſel of water upon it; this water was heated to 125°; and then laying bare the jugular vein of a living dog, I tied it properly, cut a piece of it out, and put it into this water: after eleven minutes, I took out the vein, opened it, and found the blood entirely coagulated; thence I concluded, that 125°, or leſs, was ſufficient to coagulate the blood of a dog. It may be neceſſary to obſerve here, that the part coagulated was only the lymph; for the *ſerum* requires a much greater heat to fix it, that is heat of 160°, as will appear hereafter.

EXPERIMENT VIII.

The ſame experiment was repeated in ſuch a manner, that the heat was never higher than 120° and an half; and I found, on opening the vein at the end of eleven minutes, that the lymph was entirely coagulated, even in this heat.

EXPERIMENT IX.

I next repeated the experiment, ſo that the heat was never higher than 114°, and was commonly at that degree during the eleven minutes, at the end of which time the vein being opened, the blood was found to be fluid, and in a few minutes after, being laid open to the air, it coagulated as it uſually does. Now as the blood, in the laſt experiment but one, was coagulated, when the heat had never riſen above 120° and an half; and in this experiment was fluid, though it had been expoſed to a heat of 114°; we may therefore conclude, that the coagulable lymph in the blood of a dog, in health, is fixed in a degree of heat between 114° and 120 ½ of Fahrenheit's thermometer.

As to the degree of heat at which the lymph in human blood coagulates, I have not yet had an optunity of trying it in a more ſatisfactory way, than with the mixture with Glauber's ſalt, in which ſtate it coagulates at 125°. As we find that the human blood and that of a dog jelly nearly in the ſame time, when expoſed to the air, I think it is probable, that the preciſe degree of heat at which the lymph of the human blood coagulates, is between 114° and 120° ½. I have thought of making the experiment on the umbilical cord of a recent *placenta*, which is the moſt likely way of coming at the truth.

The degree of heat, at which the *ſerum* of the blood (which ſhould not be confounded with the lymph) coagulates, is generally ſaid to be 150°; but from the trials I have made, I am inclined to believe it requires a greater heat to fix it. My experiments were made in the following manner.

EXPERIMENT X.

I took a wide-mouthed phial, containing *ſerum*, and placed a thermometer in it, and then put it into water which was kept warm by a lamp underneath; and, in making this experiment with as much accuracy as I could, I found the heat required was 160°, which is above forty degrees more than is neceſſary for the coagulation of the lymph.

As the blood is coagulable by heat, and as the heat of an animal body is increaſed in fevers, it has been ſuppoſed that the blood might be coagulated by the animal heat, even whilſt it is circulating in the blood-veſſels; but there is little foundation for ſuch a ſuppoſition, ſince the animal heat is naturally only 98 or 100°, and in the moſt ardent fever is not raiſed above 112°.

I ſhall next proceed to enquire into the formation of the inflammatory cruſt, or *ſize*, as it is called.

This remarkable appearance is frequently met with in inflammatory diſorders, and is formed by the coagulable lymph's being fixed, or coagulated, after the red particles have ſubſided. It has indeed been ſuppoſed to be formed from the *ſerum* of the blood; and an excellent writer on this ſubject ſeems in doubt to which it ſhould be attributed. But that it is formed by the coagulable lymph alone, after the red particles have ſubſided, appears from the following experiments.

EXPERIMENT XI.

In the month of June, when the thermometer in the ſhade ſtood at 67°, I bled a man who had laboured under a *phthiſis pulmonalis* for ſome months, and at that time complained of a pain in his ſide. The blood, though it came out in a ſmall ſtream, yet flowed with ſuch velocity, that it ſoon filled the baſon. After tying up his arm, I attended to the blood, and obſerved that the ſurface became tranſparent, and that the tranſparency gradually extended deeper and deeper, the blood being ſtill fluid. That the coagulation firſt began on the ſurface, where it was in contact with the air, and formed a thin pellicle; this I removed, and obſerved it was ſoon ſucceeded by a ſecond. I then took up a part of the clear liquor with a wet tea-ſpoon, and put it into a phial with an equal quantity of water; a ſecond portion I kept in the tea-ſpoon; and I found afterwards that they both jellied or coagulated, as did the ſurface of the *craſſamentum*, making a thick cruſt. On preſſing with my finger that portion which was in the tea-ſpoon, I found it contained a little *ſerum*.

From this experiment it is evident, that the ſubſtance which formed the ſize was fluid after it was taken from the vein, and coagulated when expoſed to the air; and as this is a property of the coagulable lymph alone, and not of the *ſerum*, there can be no doubt that the cruſt was formed of the former, and not of the latter.

The following experiment, made on the blood, without expoſing it to the air, likewiſe proves the ſame fact.

EXPERIMENT XII.

Immediately after killing a dog, I tied up his jugular veins near the *ſternum*, and hung his head over the edge of the table, ſo that the parts of the veins where the ligatures were might be higher than his head. I looked at the veins from time to time, and obſerved that they became tranſparent at their uppermoſt part, the red particles ſubſiding. I then made a ligature upon one vein, ſo as to divide the tranſparent from the red part of the blood; and, opening the vein, I let out the tranſparent part, which was ſtill fluid, but coagulated ſoon after. On preſſing the *coagulum*, it was found to contain a little *ſerum*. The other vein I did not open till after the blood was congealed, and then I found the upper part of the *coagulum* whitiſh like the cruſt in pleuritic blood *.

It has been a very generally received, opinion that inflammation thickens the blood, and makes it more ready to coagulate. Nay, ſome have gone ſo far as to ſay, that in thoſe diſorders where the inflammatory cruſt is ſeen, the blood is almoſt coagulated even before it is let out of the vein. Now I am perſuaded from experiment, that the contrary of this is true; or that inflammation, inſtead of increaſing the diſpoſition of the blood to coagulate, really leſſens it; and inſtead of thickening the blood, really thins it; at leaſt, that part which forms the cruſt, viz. the coagulable lymph.

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* This is not the only animal that ſeemed to be in health, whoſe blood had a cruſt; I have ſeen it in others: whence I at firſt ſuſpected that merely keeping the blood fluid for a little time was ſufficient to produce this appearance; but I altered my opinion, on ſeeing, that in the greateſt number of animals it did not occur; nor is it commonly met with in the hearts of thoſe that die a violent death, though the blood remains longer fluid in ſuch caſes than it does in the baſon where a ſize appears.

In the firſt place, that inflammation really leſſens the diſpoſition to coagulate, will appear evident to every one who attends to the jellying of ſuch blood as has a cruſt. For in all thoſe caſes the blood will be found to be longer in congealing, than it is commonly. To this opinion, I was firſt led by attending to the phthiſical patient's blood above-mentioned; but I have ſince made a compariſon, which ſeems to prove the fact. For, from a variety of experiments made on the blood of perſons nearly in health, or at leaſt who had no inflammatory complaints, and no cruſt on their blood, I found that the blood, after being taken from a vein, began to jelly in about three minutes and an half. The firſt appearance of coagulation is a thin film on the ſurface near the air-bubbles, or near the edge of the baſon; this film ſpreads over the ſurface, and thickens gradually till the whole is jellied, which is in about ſeven minutes after the opening of the vein; and in about ten or eleven the whole is ſo firm, that, if the cake be cut, the gaſhes are immediately filled up by the *ſerum*, which now begins to ſeparate from the *craſſamentum*. But in thoſe perſons whoſe blood has an inflammatory cruſt, the coagulation is much later; and in general, I believe, is lateſt in thoſe caſes where the cruſt is thickeſt, and *vice verſa.* The following experiments ſeem to prove this.

EXPERIMENT XIII.

I bled a woman who was ſeven months gone with child, and the blood was received into a baſon. In five minutes after the vein was opened, a film firſt appeared; but this ſpread ſo ſlowly, that in ten minutes it did not cover the whole ſurface: in fifteen minutes it had nearly ſpread over the ſurface; but the reſt of the blood was quite fluid, at leaſt for ſome depth, and even in half an hour it was not ſo firmly jellied as it was afterwards. In this caſe there was a very thick and ſtrong cruſt or ſize.

EXPERIMENT XIV.

Having bled a perſon with a violent rheumatic pain in his breaſt, the blood was received into three tea-cups, and each of them had afterwards a cruſt. In the firſt I obſerved the progreſs of the coagulation, as follows. The beginning of the coagulation was not marked, but at the end of half an hour the film was not thicker than common writing-paper; and this being removed, a little of the clear lymph was taken up with a wet tea-ſpoon, put into a clean cup, and was twenty minutes more in coagulating. Even at the end of an hour and an half, the whole of the blood was not jellied; for at this time I removed the film or pellicle, and took up a ſecond portion of clear lymph with a ſpoon, and put it into a tea-cup, where it jellied afterwards,

terwards; though this jelly was not indeed quite ſo firm as the *craſſamentum* itſelf.

EXPERIMENT XV.

A woman, with a ſlight inflammation in her throat, had eight ounces of blood taken from her arm; the blood was received into a baſon, and the bleeding finiſhed in four minutes and three quarters, when a film was begun to be formed near the air-bubbles; in ſeven minutes a tranſparent ſize appeared over a conſiderable part of the ſurface which was quite fluid, whilſt the reſt of the blood was coagulating, there being now a very diſtinct red cruſt over the reſt of the ſurface.

Now, from comparing theſe experiments with what has been obſerved of the coagulation of the blood, where there is no inflammatory cruſt or ſize, is it not evident that the blood remains longer fluid after being expoſed to the air, and has leſs diſpoſition to coagulate, in thoſe caſes where there is a ſize, than where there is none? for in thoſe caſes where there was none it was found to coagulate completely in ſeven minutes; but in one of the others, where the ſize was very thick, it was found not to coagulate completely in leſs than an hour and an half?

The effect that inflammation has in leſſening the lymph's diſpoſition to coagulate, is likewiſe plain from the following experiment, where the blood in the heart of a dead animal ſeems to have coagulated very ſlowly.

EXPERIMENT XVI.

A dog was killed, eight hours after receiving a large wound in his neck, The wound had during this time inflamed conſiderably. Upon opening him next morning, when he had been dead thirteen hours, a large whitiſh *polypus* was found in the right ventricle of his heart; under this was a little blood ſtill fluid, which being taken up with a tea-ſpoon, was found to coagulate ſoon after being expoſed to the air.

It may be proper to obſerve here, that in the hearts of animals which had died without any inflammation, I have found the blood entirely coagulated long before this time. And that from opening them at different times, I find it coagulates in their hearts after death, in the ſame gradual manner that it does in their veins, when its motion is ſtopt by ligatures; as related pag. 380.

In the next place, that the blood is really attenuated in inflammatory diſorders, where the whitiſh cruſt or ſize appears, is probable from the following circumſtances; firſt, it even ſeems thinner to the eye; 2dly, the red particles, or globules ſubſide ſooner in ſuch blood, than in that of an animal in health: this ſeems proved by obſerving that in the above-mentioned experiments, where the blood was at reſt in the veins, it was not covered with a cruſt, except in one or two inſtances, though in all thoſe caſes it remained longer fluid than the blood commonly does in a baſon where the cruſt appears. And again, the blood in the heart of an animal that dies a violent

lent death, is not generally covered with a white cruſt, notwithſtanding it is ſo late in being congealed. Theſe circumſtances ſhew, that ſomething more than merely a leſſened diſpoſition to coagulate is neceſſary for the forming of the cruſt or ſize. 3dly, The globules more readily ſubſide in inflammatory caſes, from the ſurface of the whole maſs of blood, than they will afterwards do from the ſurface of a mixture with the *ſerum* alone, of which the following experiments are a proof; but, before I relate them, let me obſerve, that they were made with a view to diſcover, whether that remarkable appearance, the inflammatory cruſt, could be owing to any other cauſe than to the coagulable lymph's being attenuated, and having its diſpoſition to coagulation leſſened: and as the ſame appearance might be ſuſpected to ariſe from an increaſed ſpecific gravity in the red particles, or from the *ſerum* alone being attenuated, I endeavoured to decide the queſtion by the following experiments.

EXPERIMENT XVII.

Into a phial, marked A, I put an ounce of the *ſerum* of the blood of a perſon, whoſe *craſſamentum* had an inflammatory cruſt.

Into another, marked B, I poured an ounce of the *ſerum* of a perſon whoſe blood had no cruſt; then to each of theſe, I added a tea-ſpoonful of *ſerum*, loaded with the red particles of a perſon whoſe blood had no inflammatory cruſt or *buff*. In attending to them, I could not obſerve that the red particles ſubſided at all, ſooner in the *ſerum* of the blood that had a cruſt, than they did in the *ſerum* of that blood which had no cruſt. Thence I am to conclude, that the *ſerum* is not attenuated in thoſe caſes where the inflammatory cruſt appears.

To ſee whether the ſpecific gravity of the red globules was increaſed, I tried as follows.

EXPERIMENT XVIII.

I poured into a phial C a portion of the *ſerum* of the blood which had no cruſt; and likewiſe into another D a ſecond portion of the ſame *ſerum*. I then added to C a tea-ſpoonful of the ſame *ſerum*, loaded with red particles from the blood which had an inflammatory cruſt. And into D I poured a tea-ſpoonful of the ſame *ſerum*, loaded with the globules of blood which had no cruſt. In viewing theſe, I could not obſerve that the globules of the blood which had an inflammatory cruſt ſubſided at all, ſooner than thoſe of the blood which had none; thence I am inclined to conclude, that the ſpecific gravity of the red particles, or globules as they are called, is not increaſed in thoſe caſes where the cruſt appears. And, therefore, ſince that inflammatory cruſt or ſize, ſeems neither owing to the *ſerum's* being attenuated, nor to an increaſed ſpecific gravity in the red particles, it is probably intirely owing to a change in the coagulable lymph. And, what ſeems farther to confirm this inference, in none of theſe experiments did the red particles ſubſide from the ſurface of the *ſerum* in 20 minutes, though where the cruſt appears, they ſubſide from the ſurface of the whole maſs of blood in half that time; ſo that the whole maſs of blood ſeems to be thinner than the *ſerum* alone; or, the coagulable lymph ſeems to be ſo much attenuated in theſe caſes, as even to dilute the *ſerum*, which at firſt ſight appears a paradox.

May we not, therefore, now conclude, that in thoſe caſes where the inflammatory cruſt appears, the coagulable lymph becomes thinner, and its diſpoſition to coagulation is leſſened; both of which circumſtances contribute to the ſubſiding of the red globules before the ſurface of the blood is coagulated, and thence give riſe to this appearance, called the inflammatory cruſt or ſize *?

How contrary to the concluſion, which theſe experiments lead us to, are the opinions of ſome medical writers on this ſubject! How frequently do we find it ſaid, that the blood is thicker in inflammatory diſorders, where that appearance occurs; and that a large orifice is neceſſary to let out the vitiated blood! That a large orifice is preferable to a ſmall one in many caſes, where ſuch blood is found, I believe is a truth: but that its advantages are owing to its letting out the thickened blood, ſeems improbable from what we have ſeen in the experiments above related: they are perhaps nearer the truth, who attribute it to the ſuddenneſs of the evacuation.

It may be proper to obſerve here, that this ſize or whitiſh cruſt, is not a certain ſign of inflammation; it is often met with where there ſeems to be no ſuch diſeaſe, in particular in the blood of pregnant women. And we may likewiſe obſerve with Sydenham, that even in thoſe caſes where the blood has a diſpoſition to form a white cruſt, yet if it trickle ſlowly from the vein, this *ſize* will not appear; for in ſuch caſes, probably, the blood begins to coagulate before the whole has done flowing, ſo that the agitation prevents the red particles from ſubſiding from the ſurface. There are therefore ſeveral circumſtances to be taken into the account, before we judge, from the preſence or want of this *ſize*, whether there is, or is, not an inflammation.

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* This remarkable appearance might indeed be accounted for, by ſuppoſing that the lymph had aſcended to the ſurface of the blood in thoſe caſes; but this is improbable, from conſidering, that, in its coagulated ſtate, it is of greater ſpecific gravity than the ſerum, and ſinks in it.

The whitiſh cruſt differs much in denſity in different caſes; in ſome it is extremely denſe, in others it is ſpongy or cellular, and contains a quantity of *ſerum* in its cells.

Received July 7, 1770.

XXXIV. *Further Remarks on the Properties of the Coagulable Lymph; on the ſtopping of Hæmorrhages; and on the Effects of Cold upon the Blood: By* the ſame.

Read Nov. 22, and Dec. 6, 1770.

IT has been obſerved by thoſe who have written on the blood, that it ſometimes happens in blood-letting, that the firſt cup has an inflammatory cruſt, whilſt the laſt has none; but no ſatisfactory reaſon has been given for this appearance. One might ſuppoſe that it was owing to a difference in ſome circumſtance in the operation, ſuch as in the velocity with which the blood flowed into each cup, or by the latter being agitated ſo as to prevent the ſeparation of the lymph: but I have ſeen it where there was no difference of this ſort, nor in any other circumſtance that I could obſerve. I have therefore ſuſpected that in ſuch caſes the properties of the blood are changed, even during the time of the evacuation; and in this opinion I am confirmed by the following experiments.

EXPERIMENT XIX.

Nine ounces of blood were taken from a woman who had been delivered two days before, and who at that time laboured under a fever, with a conſiderable pain in her ſide, and in her *abdomen*. The blood was received into a baſon, and her arm was tied up; when, on looking at the blood, I found its ſurface tranſparent for ſome depth, an indication of a future cruſt; and as her pain was not abated, and her pulſe bore it well, I removed the ligature from her arm, and took away about ſix ounces more, into three tea-cups; but what appeared to me remarkable, although the blood flowed as faſt into each of the cups as into the baſon, and they were ſet down immediately when filled on the ſame window, yet there was no inflammatory cruſt on that in the cups, though a very denſe one on that in the baſon. And again, although that in the baſon was taken away ſome minutes before that in the cups, yet it was later in being completely coagulated; as was evident on comparing them.

I had an opportunity of repeating the experiment in the evening; for her ſymptoms of inflammation ſeeming equally violent, it was thought proper by the phyſicians who attended her, to take away more blood; which was done by opening the ſame orifice, when three tea-cups were nearly filled, and ſet in the ſame place; and it was obſerved, that the firſt had a cruſt, though not ſo thick a one as in the firſt operation; but the other two cups were without this appearance, though the blood had flowed into them even more quickly than into the firſt.

EXPERIMENT XX.

A gentleman, who laboured under an inflammatory complaint, had about nine ounces of blood taken from his arm. This quantity was divided into four portions; the firſt was received in a cup, and was in meaſure little more than an ounce; the ſecond, in a baſon, to the quantity of two ounces; the third, in a cup, which held one ounce; and the fourth, in a baſon, to the quantity of three ounces. Each veſſel was placed immediately upon the window; and it was obſerved that the blood in the firſt was lateſt in coagulating, and had a cruſt over the whole ſurface; that in the ſecond, had a cruſt only upon a part of its ſurface; but that in the third and fourth had no cruſt, and manifeſtly coagulated before either of the other two.

From theſe experiments it is evident, that the properties of the coagulable lymph can be ſoon changed; ſince, in the ſpace of three or four minutes, the diſpoſition to coagulate was increaſed, and probably too the lymph was thickened. It might indeed, at firſt ſight, ſeem poſſible, that blood-letting had only let out the vitiated part; but this is highly improbable; for, ſuppoſe a part only of the blood was vitiated, that part muſt have been equally diffuſed through the whole maſs, and there is no likelihood of its getting out of the veſſels before the reſt of the blood; and conſequently it ought to have appeared in the laſt equally as in the firſt cup, but it did not. Bleeding, therefore, in thoſe caſes alters the nature of the blood, not by removing the vitiated part, and giving room for new blood to be formed, as has been ſuſpected; but probably by changing that ſtate of the blood-veſſels on which the thinneſs, and leſſened tendency of the lymph to coagulation, depends; which ſurely is a very curious circumſtance. This fact is the more remarkable, ſince it ſhews the improbability of the opinions of thoſe who maintain that this vitiated blood is the cauſe of the diſeaſe, ſince the diſeaſe remained, though the properties of the blood were changed *.

From this obſervation we may be led to think, that it may be uſeful to receive the blood more frequently into ſmall cups, inſtead of a baſon, and to attend more carefully to the alteration produced upon it by bleeding; as we may by that means perhaps learn to determine better, what quantities ſhould be taken away in particular caſes. For it would ſeem probable that the operation is likely to have the moſt effect on the diſeaſe, in thoſe caſes where the greateſt change is produced by its means, on the diſpoſition of the blood to coagulate; and of that change, we can judge, by comparing the blood in the firſt cup, with that in the laſt; for the firſt cup will nearly ſhew the ſtate of the blood at the beginning; and the laſt cup the ſtate of the blood at the latter part of the evacuation.

It frequently happens, that inſtead of an inflammatory cruſt over the whole ſurface of the *craſſamentum*, there is only a partial one, which appears in large ſpots or ſtreaks. In ſuch caſes I have obſerved, that only a part of the blood had its diſpoſition to coagulate leſſened, as in experiment XV. in which ſome of the blood remained fluid and tranſparent, where thoſe ſtreaks appeared, for ſome time after the coagulation had begun in other parts of the ſurface. Now whether in thoſe caſes there had been the ſame difference before the vein was opened, or whether the whole blood had not been of the inflammatory kind, before veneſection, and a part of it was changed as it ran out, or as ſoon as the general fulneſs was diminiſhed, may be a queſtion; but the probability, I think, is much in favour of its being changed during the time of the evacuation, from what was obſerved in the laſt related experiments.

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* That the properties of the blood can be changed by emptying the blood-veſſels, is likewiſe proved by an experiment hereafter to be related; where the blood in an animal in health was found to have its diſpoſition to coagulation increaſed, in proportion as the veſſels were emptied, and as the animal became weaker. It may likewiſe be neceſſary to mention, that though the inference is here drawn from two experiments only, yet I have likewiſe obſerved the ſame appearance in other caſes, which I have thought it unneceſſary to relate.

When I had obſerved that this diſpoſition of the lymph to coagulate was increaſed by bleeding, or by weakening the action of the blood-veſſels, it occurred to me, that poſſibly in thoſe caſes where the body was very weak, the diſpoſition to coagulate might be ſo much increaſed, that inſtead of being three or four minutes in beginning to do it, after it is let out of the veins (as is the caſe in people in health) it might coagulate in leſs time, or almoſt inſtantaneouſly; for I imagined, that unleſs this took place, we could hardly conceive how the blood ſhould ever have time to coagulate in ruptured veſſels, ſo as to ſtop hæmorrhages, as it is believed to do. And upon this occaſion I recollected an obſervation of Dr. Hunter's, which is "that the faintneſs which comes "on after hæmorrhages, inſtead of alarming the by-"ſtanders, and making them ſupport the patient by "ſtimulating medicines, as ſpirits of hartſhorn and "cordials, ſhould be looked upon as ſalutary; as it

" ſeems to be the method nature takes to give " the blood time to coagulate!" As this obſervation ſeemed to favour my ſuſpicion, I determined to make the experiment.

EXPERIMENT XXI.

Believing it would be ſufficient for this purpoſe, to attend to the properties of the blood, as it flows at different times from an animal that is bleeding to death, I therefore went to the markets, and attended the killing of ſheep; and having received the blood into cups, I found my notion verified. For I obſerved, that the blood, which came from the veſſels immediately on withdrawing the knife, was about two minutes in beginning to coagulate; and that the blood taken later, or as the animal became weaker, coagulated in leſs and leſs time; till at laſt, when the animal became very weak, the blood, though quite fluid as it came from the veſſels, yet had hardly been received into the cup before it congealed. I have alſo varied the experiment, by receiving blood into different cups at different times, whilſt the animal was bleeding to death; and though the time taken up in killing the animal was not commonly more than two minutes, yet I obſerved, on comparing the cups, that the blood which iſſued laſt coagulated firſt. I have obſerved likewiſe, that the blood coagulates with a different appearance in proportion as the animal becomes weaker; that which follows the knife begins to coagulate in about two minutes; it firſt forms a film or pellicle on the ſurface, which extends gradually through the whole blood, yet ſo ſlowly that its progreſs may be obſerved, eſpecially if the pellicle be moved from time to time. But the blood that flows from the fainting animal is coagulated in an inſtant, after it once begins. From this obſervation, that the diſpoſition of the blood to coagulate is increaſed as the animal becomes weaker, we may draw an inference of ſome uſe with regard to the ſtopping of hæmorrhages, viz. not to rouze the patient by ſtimulating medicines, or by motion, but to let that languor or faintneſs continue, ſince it is ſo favourable for that purpoſe; and alſo, that the medicines likely to be of ſervice in thoſe caſes, are ſuch as cool the body, leſſen the force of the circulation, and promote that languor or faintneſs *. For in proprtion as theſe effects are produced, the divided arteries become more capable of contracting, and the blood more readily coagulates; two

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* Beſides giving ſtimulants and cordials to counteract the fainting, it is a common practice in many parts of England, to give women, who are flooding, conſiderable quantities of port-wine, on a ſuppoſition that it will do them ſervice by its aſtringency. But ſurely, from its increaſing the force of the circulation, it muſt be prejudicial in thoſe caſes. Perhaps many of the remedies called ſtyptics might be objected to for the ſame reaſon.

It has of late been proved by experiments, particularly by thoſe of the ingenious Mr. Kirkland, that the larger arteries, when divided, contract ſo as to ſtop the hæmorrhage. But the large *coagula* which we ſee in the orifices of the veſſels of the *uterus* of thoſe who die ſoon after delivery, and the ſtopping of hæmorrhages where the blood-veſſels were ruptured on their ſides and not entirely divided, makes me believe that contracting the bleeding orifice is not the only method nature takes to ſtop an circumſtances that ſeem to concur in cloſing the bleeding orifices.

It has been queſtioned whether blood-letting can be properly recommended in hæmorrhages, excepting in thoſe that are attended with evident ſigns of *plethora:* but do not theſe experiments ſhew, that a vein may be opened with propriety, even where there is no *plethora*, in order ſuddenly to bring on weakneſs; by which the momentum of the blood may be ſo diminiſhed, and the diſpoſition of the lymph to coagulate may be ſo increaſed, as to ſtop the hæmorrhage? For, when we conſider how ſoon the blood-veſſels contract, and adapt themſelves to the quantity of blood which they contain, it ſeems not improbable that in ſome caſes where the hæmorrhage is not profuſe, but long-continued, the ſtrength of the patient may be ſo recruited, that the diſpoſition to coagulate ſhall not be ſufficiently increaſed, or the extremities of the veſſels ſufficiently contracted, for the ſtopping of the bleeding; but, by emptying the veſſels ſuddenly, this effect may be produced, and the hæmorrhage may be ſtopt by the loſs of leſs blood, than would have happened, had only the ſlow draining been continued.

Although the whitiſh cruſt is ſo commonly ſeen in inflammatory diſorders, and has ſo very morbid an appearance, as might induce us to conſider it as inflammatory, and to bleed repeatedly in all thoſe caſes where it occurs, yet I believe we ſhould act improperly: for, to ſay nothing of pregnancy, in

hæmorrhage. Her reſources indeed are great, and ſhe has often more methods than one of producing the ſame effect.

which the appearance is almoſt conſtant, there are few phyſicians that have not ſeen patients, who, even in ſuch circumſtances, have been the worſe for this evacuation. Nor need we be ſurpriſed that this ſhould happen, conſidering how ſoon in ſome inſtances this ſize diſappears; and if ſo, may we not ſuppoſe, that it may likewiſe ſoon be formed, even by a ſhort exertion of ſtrength in the veſſels? Perhaps this was the caſe in the gentleman mentioned in page 399, who, in leſs than twenty-four hours after bleeding, had ſymptoms of great weakneſs.

As it appears from the above related experiments, that the diſpoſition of the blood to coagulate is increaſed by bleeding, it may perhaps be uſeful to attend to this circumſtance, and to compare the coagulation of the blood in the laſt, with that in the firſt cup, even in caſes that are not attended with the inflammatory cruſt. And it may likewiſe be worth while to make the ſame compariſon in thoſe caſes where every cup has a cruſt; which frequently happens in rheumatic, and likewiſe in phthiſical complaints. By theſe means we may judge what effect the evacuation has produced on the ſtrength or fulneſs of the veſſels; and may perhaps, by attending to the laſt cup, if it contain only a ſmall quantity, be able to gueſs pretty nearly at the nature of the blood which remains in the body. In the rheumatic caſe mentioned in page 391, every cup contained this cruſt; and although the blood in the laſt cup coagulated in much leſs time than that in the firſt, yet as it was later in coagulating than common. I ſuſpected what remained in the veſſels had the ſame

diſpoſition;

diſpoſition; but the patient got well without repeating the evacuation.

It may be mentioned here, that I have once or twice ſeen blood, which, when it firſt began to coagulate, had on its ſurface a red pellicle, and underneath a tranſparent fluid, which afterwards formed a cruſt. In theſe caſes, if the red pellicle had not been removed before the reſt of the blood had congealed, we might have concluded that no part of the blood had this diſpoſition to form a white cruſt. This appearance, I ſhould imagine, was owing to the blood, where in contact with the air, having coagulated very early, and therefore before the red particles had time to ſubſide, from that part of the lymph which had its diſpoſition to coagulation leſſened.

The learned profeſſor de Haen has taken notice of a curious appearance of the blood, which he could not account for; but which, I preſume, may be explained from ſome of the above experiments. His obſervation is, "That, having bled a perſon in a fever, "the blood was covered with an inflammatory cruſt, "and upon examining the *craſſamentum* in one of "the cups, it formed a ſort of ſack containing a "clear fluid: this fluid being let out, and the whole "covered and ſet by, on examining it next morning, "a very firm cruſt covered the whole again, and "extended to the bottom of the cup*." I once met with a caſe ſimilar to this; for, having bled a perſon into four cups at ten o'clock in the morning, on looking at the blood afterwards, at five in the afternoon, I found the *ſerum* had not ſeparated from the *craſſamentum* in the firſt cup; but the

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* Vide Rat. Medendi, cap. vi.

craſſamentum felt as if it contained a fluid in a bag, as profeſſor de Haen has deſcribed it. Upon preſſing it, the fluid guſhed out, which, in a few minutes after being expoſed to the air, coagulated: there was however this difference in the two caſes, that in mine the fluid was red, ſo that it formed a red cruſt over the firſt, which was white. Now this ſeems to have been owing to the blood's having firſt coagulated, where it was in contact with the air and with the ſides of the cup; and the fluid which guſhed out was the *ſerum*, with a part of the coagulable lymph, which yet remained fluid; but, when expoſed to the air, jellied or coagulated, as it naturally does. That one part of the lymph can remain fluid after the other is coagulated, is proved by ſome of the preceding experiments; and I have more than once ſeen blood, which appeared perfectly jellied ſoon after bleeding; yet, on cutting into the *coagulum*, a tranſparent fluid has ouzed out, which afterwards jellied. And ſo ſlowly does this coagulation proceed in ſome caſes, that, in an experiment mentioned before, a part of the blood in a dog's heart was found uncoagulated thirteen hours after death. And I have likewiſe diſtinctly obſerved, that in ſome caſes where the diſpoſition to coagulate was much leſſened during the evacuation, the blood at the bottom of the cup has jellied, whilſt the greateſt part of the *ſize* at the top was yet fluid; there being only a thin pellicle on its ſurface, where it was in contact with the air.

Another inſtance of a change in the properties of this coagulable lymph, which appears curious, was obſerved in ſome experiments, where I had occaſion to throw the blood into water, and oil, during the winter ſeaſon whilſt the heat of the water and oil was no higher than 41° of Fahrenheit's ſcale. In all thoſe experiments, I found that the diſpoſition to coagulate was leſſened, the blood becoming more and more viſcid, but did not coagulate whilſt in that degree of cold. I ſhall next relate thoſe experiments.

EXPERIMENT XXII.

The jugular vein being cut out from a rabbit juſt killed, was thrown into water of 41° of heat, and taken out at the end of half an hour; when the blood was found to be ſtill fluid, though rather more viſcid than natural; but, after being expoſed to the air for a few minutes, it coagulated.

EXPERIMENT XXIII.

Two pieces of the jugular vein of a dog, juſt killed, were put into water, in which the thermometer ſtood at 41°; one was taken out after twenty minutes, and the other after three quarters of an hour; the blood in both was found to be fluid, and to coagulate afterwards.

As it was evident, from theſe experiments, that the water had leſſened the diſpoſition of the blood to coagulate, I next enquired to what property in the water this effect could be owing; and to ſee whether water that was warmer would not have the ſame effect, I made the following experiment.

EXPERIMENT XXIV.

On December the thirteenth, I cut out two pieces of the jugular vein of another dog, immediately after his death. One piece was put into cold water, and the other into water kept warm by a lamp, ſo that the heat never varied more than between 90 and 100°. At the end of three quarters of an hour, that in the warm water had in it a coagulum as large as a garden pea; but that in the cold water, on being let out into a cup, was found to be quite fluid. Twenty minutes after being expoſed to the air, that which had been in the cold water was coagulating; but that from the warm water neither then nor afterwards ſhewed any ſigns of farther coagulation: ſo that it ſeemed not only to have jellied whilſt in warm water, but to have begun to part with its *ſerum*. From this experiment, it ſeems probable that the *coldneſs* was that property of the water to which the leſſened diſpoſition to coagulate was owing; but, to be more ſure of this, and to ſee whether the blood might not be kept fluid a longer time by theſe means, I tried as follows.

EXPERIMENT XXV.

On January the fourteenth, I cut out a piece of the jugular vien of another dog, and put it into oil, in which the thermometer ſtood at 38°. At the end of ſix hours it was taken out, and the red particles were obſerved through the coats of the vein to have in great meaſure ſettled to one ſide.

The blood was let out into a cup, and was found to be fluid; at the end of fifteen minutes above one half was ſtill fluid; in twenty-five minutes it ſeemed to be quite jellied. Now as in this experiment a ſimilar effect was produced, as when the vein was put into water, it ſeems probable that it was the coldneſs of the water, and of the oil, which had leſſened the diſpoſition of the lymph to coagulate.

EXPERIMENT XXVI.

Another piece of the ſame vein was put into river-water, in which the thermometer ſtood at 38°, and was left till the next morning; when, after twenty-two hours and a quarter, it was taken out. The particles did not ſeem to have ſubſided, as in the former experiment; but the vein being opened the blood was found to be fluid, though ſo viſcid that it could barely drop from the vein. The cup into which it was received was placed upon the window of a moderately warm room, and was examined carefully from time to time; but the blood never had any appearance of coagulation, but remained fluid till it was dried by the evaporation of its water, which happened by the next day. In this experiment the cold ſeemed intirely to have prevented the coagulation of the lymph: ſo ill-founded is the common opinion, that cold coagulates the blood.

As the lymph can be deprived of its power of coagulating, on being expoſed to the air, by ſo ſlight a circumſtance, as it would ſeem, as the cooling it ſuddenly; it is the leſs to be wondered at, that ſuch a change ſhould ſometimes take place in the blood-

veſſels

veſſels of a living body; an inſtance of which, I have been told, was obſerved by the learned profeſſor Cullen, who having ordered an epileptic patient to be bled, the blood was found not to coagulate; but, on bleeding him the day following, the blood coagulated, as uſual. A ſimilar inſtance, I ſaw lately, by the favour of the medical gentlemen of the Britiſh Lying-in-Hoſpital, who, having bled a woman in a fever that came on ſoon after delivery, the blood was found not to coagulate on being expoſed to the air, but appeared like a mixture of the red particles and ſerum only; the particles having ſubſided to the bottom, in the form of a powder. She died three days afterwards; and, upon opening her, we found the blood had coagulated in her veſſels after death; and that a denſe white *polypus* was formed in each auricle of her heart; one of which, I have now by me. I examined the blood taken away before ſhe died, and found on expoſing it to heat, that it did not coagulate ſooner than *ſerum* commonly does, or under 160°: ſo that it is probable, that at the time ſhe was bled, her blood either was without the coagulable lymph, or that its properties were changed.

After a blow or contuſion, the blood now and then burſts from the blood-veſſels into the cellular membrane, ſometimes forming an *ecchymoſis*, and ſometimes a tumor; and it is a queſtion with ſome, whether that blood coagulates or not; but that it coagulates in moſt of theſe caſes, is proved by the opening of ſuch tumors. Yet it has likewiſe been obſerved, that now and then theſe tumors have been attended with a fluctuation, and after ſome time their contents have been abſorbed: and it has alſo been found upon opening ſome of them, even ſeveral weeks after the accident, that the blood was fluid. In ſuch caſes the blood had probably undergone a change ſimilar to what was obſerved to take place in ſome of the preceding experiments; that is, the lymph had been deprived of its property of coagulating, in paſſing from the blood-veſſels into the tumor.

XXXV. *Account of ſome Bones found in the Rock of* Gibraltar, *in a Letter from* John Boddington, *Eſq*; *to Dr.* William Hunter, *F. R. S. with ſome Remarks from Dr.* Hunter *in a Letter to Dr.* Mathew Maty, *M. D. Sec. R. S.*

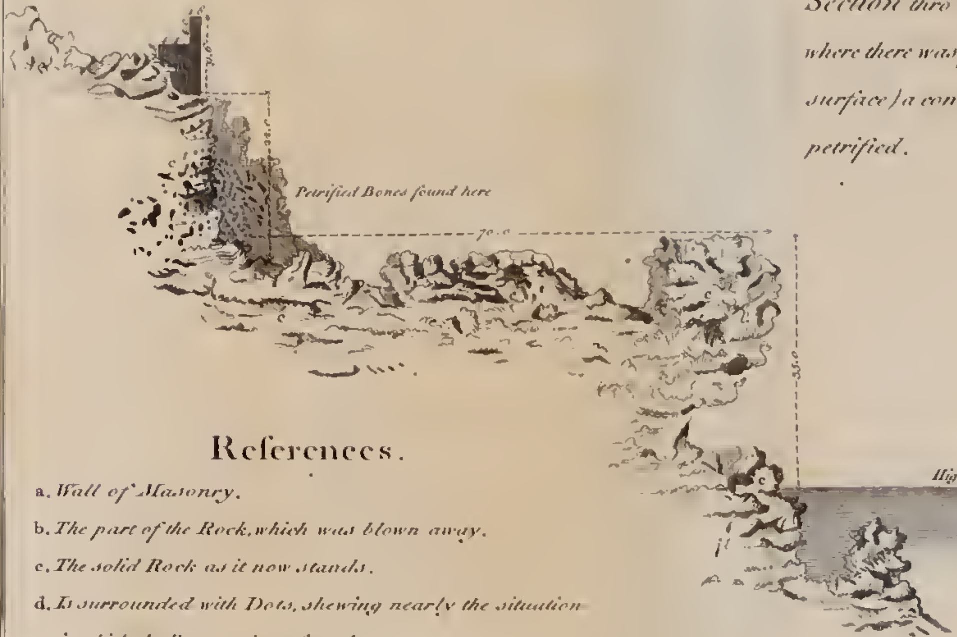
Dear Sir,

Read Feb. 1, 1770.

I BEG your acceptance of a piece of the rock of Gibraltar, which my friend Colonel Green, chief engineer of that garriſon, has brought from thence, and given to me as a natural curioſity: it appears to me a very extraordinary one indeed; therefore, I ſhall attempt to explain to you the manner of diſcovering it, and leave the reſt to your better judgment.

You muſt know then, Sir, that Gibraltar is always attended to with great circumſpection. The city, town, and fortification are all upon a rock, and ſand; of which the whole peninſula is compoſed: as nature changes the face of the rock, the engineers have a watchful eye to apply art in forming the defences where nature fails; a particular inſtance of which happened in the courſe of the preſent year, by the craggy part of the rock falling away, ſo as to admit the probability of an entrance into the fortification;

Section thro a solid part of the Rock at Gibraltar *where there was found (about ten feet from the nearest surface) a considerable Quantity of Bones quite petrified.*



References.

a. *Wall of Masonry.*
b. *The part of the Rock, which was blown away.*
c. *The solid Rock as it now stands.*
d. *Is surrounded with Dots, shewing nearly the situation in which the Bones where found.*

NB. The Rock near where the Bones are, is Free Stone of a Reddish Colour intermixed with Blue Lime Stone, of which the rest of the Rock is composed.

J. Mynde sculp.

tion; to obſtruct which, the wall marked *a*, in the incloſed ſketch, TAB. X. was erected 70 feet diſtant from the ſea ſhore, and 57 feet perpendicular above high water mark. In blowing up the rock to make way for the foundation of the ſaid wall, there was diſcovered conſiderable quantities of petrified bones, as you may perceive upon examining the piece of rock, which you may be certain was taken from the ſpot by Colonel Green, and has been in the poſſeſſion of no perſon but himſelf, till delivered to,

DEAR SIR,

Your much obliged,

and moſt obedient

humble ſervant,

Tower,
17 Dec. 1769.

John Boddington.

DEAR SIR,

BY the examination of two pieces of the rock of Gibraltar, which are in my poſſeſſion, I find that they are not, what I, at firſt, took them to be, *human* bones, but thoſe of ſome quadrupede. I diſcovered this, with my brother's aſſiſtance, by clearing the teeth of the cruſt that covered them, ſo as to ſee their ſhape more diſtinctly.

The two maſſes of bones are blended with pieces of the marble, of which the whole rock of Gibraltar,

tar, as I am informed, is compoſed; and all the conſtituent pieces are cemented ſtrongly together with a browniſh-coloured calcareous cryſtallization, or ſtalactite. Where the interſtices are large, there are vacant ſpaces; and the ſurfaces of all ſuch cavities are covered with granulated cryſtallization about $\frac{1}{6}$ of an inch thick.

This cryſtallized cruſt, no doubt, was depoſited from the water paſſing through the cavern in which the bones had been lodged; and by ſoaking through the porous ſubſtance of every bone, the water had likewiſe depoſited a cruſt of the ſame nature, but much thinner, on all the internal ſurfaces of the hollow and ſpongy bones. The bones were not in any other ſenſe petrified.

I am,

Dear Sir,

moſt faithfully yours,

William Hunter.

Received February 8, 1770.

XXXIV.* *Difficulties in the* Newtonian *Theory of Light, conſidered and removed, by the Rev.* S. Horſley, *LL. B. F. R. S.*

Read Dec. 20, 1770.

DR. Franklin, in a letter to a correſpondent at New-York, the 23d of that valuable collection with which the public was obliged in the latter end of the year 1768, propoſes ſome *objections to the Newtonian theory of light.* His words are theſe. " I am much in the dark about " light. I am not ſatisfied with the doctrine that " ſuppoſes particles of matter called light, continual" ly driven off from the ſun's ſurface with a ſwiftneſs " ſo prodigious. Muſt not the ſmalleſt particle con" ceivable have, with ſuch a motion, a force exceed" ing that of a twenty-four pounder diſcharged from " a cannon? Muſt not the ſun diminiſh exceedingly " by ſuch a waſte of matter, and the planets recede " to greater diſtances by the leſſened attraction? Yet " theſe particles with this amazing motion will not " drive before them, or remove, the leaſt and lighteſt " duſt they meet with: and the ſun, for aught we " know, continues of his antient dimenſions, and his " attendants move in their antient orbits." Dr.

Franklin's queſtions are of ſome importance, and deſerve a ſtrict diſcuſſion.

Upon the ſuppoſition that light is a copious emanation of innumerable ſmall particles of matter from the ſun, I had once occaſion to enquire, what the force of motion produced in every ſuch emiſſion could poſſibly amount to at the utmoſt.

For this purpoſe, I made an eſtimate of the greateſt probable magnitude of the particles of light; and of the greateſt denſity of each.

I likewiſe computed the greateſt number of ſuch particles, that could poſſibly fly off at once from the ſurface of the ſun; ſuppoſing the ſun's horizontal parallax to be no more than 8″.

Theſe computations, with an account of the principles on which they were founded, having been already given to the public[a]; I ſhall make uſe of the reſults (which I ſhall here briefly ſtate) as data for the diſcuſſion of Dr. Franklin's queſtions.

I ſuppoſe the particles of light to be equal ſpherules. This, perhaps, is not the caſe. Each color has probably its own ſize; but, there will be a mean ſize, which is ſufficient for my purpoſe.

This mean ſize I ſuppoſe to be ſo ſmall, that the diameter of each ſpherule does not exceed one millionth of one millionth of an inch. I ſhall ſhew hereafter, that there is much reaſon to ſuppoſe, that the particles of light are in fact much leſs than ſpherules of this diameter.

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[a] In a little treatiſe, entitled, The Power of God, deduced from the computable inſtantaneous Productions of it in the Solar Syſtem.

I ſuppoſe the denſity of each particle three times that of iron[b]. The number of ſuch ſpherules that contain as much matter as an iron ball of one yard diameter, I have found to be 15552 xxxvi[c][d]; conſequently 576 xxxvi ſuch ſpherules contain as much matter as an iron ball of 1 foot diameter.

Let ſuch a ball be ſuppoſed to move uniformly with a velocity that ſhould carry it 1000 yards in 1″.

The light of the ſun traverſes the ſemidiameter of the orbis magnus in 7′ nearly.

In the enſuing calculations, I ſhall reckon the ſun's horizontal parallax 9″.

According to this hypotheſis, the ſemidiameter of the orbis magnus will contain 22919 ſemidiameters of the earth.

In 1″ of time, light, according to the velocity aſſigned to it, traverſes $\frac{1}{420}$th of this ſpace, or 54,57 ſemidiameters of the earth, or 381092323 London yards.

Hence the velocity of each particle of light, will be to that of the iron ball moving 1000 yards in 1″ as 381092 to 1, very nearly.

And the ball being 1 foot diameter, it has been ſhewn that the matter in each particle is to the matter in the ball, as 1 to 576 xxxvi.

Hence the force of the motion in each particle of light, is to the force of motion in the ball as 381092 to 576 xxxvi; that is, as 1 to 1511444 xxvii, or, it is equal to the force of motion in an iron ball of 1 foot diameter moving $\frac{1}{1511444\ \text{xxvii}}$th of 1000 yards in 1″, or to that of an iron ball of 1 inch diameter moving $\frac{1}{874679\ \text{xxi}}$th of 1 yard in 1″, or to that of an iron ball of ¼ of an inch diameter moving $\frac{1}{13666\ \text{xxi}}$th of 1 yard in 1″; that is, moving leſs than $\frac{1}{4555\ \text{xxi}}$ of 1 foot in 1″; that is, moving leſs than a foot in 4555 xxi ſeconds, or in more than *one hundred forty four thouſand millions of millions of Egyptian years:* or the force of motion in each particle of light coming from the ſun, is leſs than that in an iron ball of ¼ of an inch diameter, moving at the rate of leſs than an inch in 12 thouſand millions of millions of Egyptian years.

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[b] Inſtantaneous Product. &c. p. 30.

[c] Ibid.

[d] The Roman numerals placed after the Arabic characters, denote the number of zeros that muſt be added to the Arabic figures, to compleat the expreſſion of the number intended.

Dr. Franklin's firſt queſtion is anſwered. A particle of matter, which is probably larger than any particle of light, moving with the velocity of light, has a force of motion, which, inſtead of exceeding the force of a twenty-four pounder diſcharged from a cannon, is infinitely leſs than that of the ſmalleſt ſhot diſcharged from a pocket piſtol, or leſs than any that art can create.

I proceed to the other queſtions—And, I think that I ſhall make it appear, that it is very poſſible that light may be produced by a continual emiſſion of matter from the ſun, without any ſuch waſte of his ſubſtance as ſhould ſenſibly contract his dimenſions, or ſenſibly alter the motions of the planets, within any moderate length of time.

Indeed, I do not think it neceſſary to the production of any of the phænomena of light, that the emanation from the ſun ſhould be *continual* in a ſtrict mathematical ſenſe, or without any interval. It ſeems ſufficient to all purpoſes, that the intervals ſhould be exceeding ſhort. But this I only mention.—I think it poſſible that a *continual* emanation might ſubſiſt, without any ſuch dangerous conſequences to the ſolar ſyſtem, as Dr. Franklin apprehends. Dr. Franklin's character is not more diſtinguiſhed by his ſuperior talents, than by a candor truly philoſophical. And upon this circumſtance, I build the ſtrongeſt confidence, that he will not be offended, that I differ from him: that, as a friend to enquiry, he will be pleaſed that I take the liberty to communicate my own notions, however oppoſite they may be to his.

It will be eaſily underſtood, that a continual emanation from the ſun does not neceſſarily imply a continual waſte, or loſs, equal to the emanation.—If light is continually iſſuing from the ſun in all directions, part of this is continually returning to him, by reflection from the planets, and other light is continually coming to him, from the ſuns of other ſyſtems. It is true, that the light which he receives, is but a very ſmall part of the light which he gives. For if the light always *coming* to the ſun were equal to the light always *going from* him, our atmoſphere would be as ſtrongly enlightened in the night as in the day.—But this is not the caſe; and the proportion of the light that *comes*, to the light that *goes*, cannot be greater than that of night-light at a medium, to day-light at a medium—ſtill it is ſomething—and the continual loſs of ſubſtance that the ſun ſuſtains cannot be more than the difference between the light that he gives, and the light that he receives.

And therefore, if there were no other recruit of the ſun's ſubſtance (which is by no means a probable ſuppoſition) yet the continual waſte will, on this account alone, be leſs than the continual emiſſion; and the ſun cannot loſe ſo much of his ſubſtance as a ſingle emiſſion of light contains, but in ſome determinate time. I ſhall ſuppoſe that the ſun gives ſo much more than he receives, that he loſes the amount of one emiſſion in every ſecond of an hour. Let us ſee what will be the conſequence. Every particle of light that iſſues from the ſun, muſt leave a ſpherical vacuity of one millionth of one millionth of an inch diameter.

The greateſt number of particles of this ſize that can iſſue from the ſurface of the ſun at once, if the horizontal parallax be 9″, is 104666 XLI[c], becauſe this is the greateſt number of ſuch particles that would have room to lie at once upon his ſurface. And the ſame will be the greateſt number of ſpherical vacuities made by one emiſſion.

Many of theſe vacuities are no ſooner made, than they are filled up by the light that is coming to the ſun from other ſyſtems, or returning to him from the bodies of his own, or, perhaps with other matter which he may receive in various emanations from the planets; for I ſtrongly ſuſpect that a perpetual circulation of finer matter may ſubſiſt between all the large bodies of the univerſe. The emiſſion of light, however, is ſuppoſed ſo far to exceed the whole ſupply, that in every ſecond, over and above the vacuities that have been filled up with adventitious matter, the foregoing number, 104666 XLI, have been formed that have received no ſuch ſupply. The *fluid* matter of the ſun[f] ruſhes from all ſides into theſe, and fills them up to its own level. The ſun by this means ſhrinks a little, and loſes, once in every ſecond, ſo much of its ſolid content, as the ſolid contents of theſe vacuities amount to.

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[c] The greateſt number of particles that can iſſue from the ſun at once, was reckoned in the Inſtant. Product. 132467 XLI; but then the ☉'s parallax was reckoned only 8″.

I have found that the ſolid contents of 46656 XXXVI ſuch vacuities are equal to the ſphere of one yard[g].

Therefore the ſolid contents of 104666 XLI ſuch vacuities, are equal to 224335½ times the ſphere of one yard. And ſo much is the ſun's ſolid content diminiſhed every ſecond.

The ſphere of the earth is to the ſphere of one yard, as 27247031 XIV to 1[h].

Therefore the ſphere of the earth is to 224335½ times the ſphere of one yard as 121456 XI to 1.

Therefore the ſun loſes *an earth* of its ſolid content in 121456 XI ſeconds, or 385130000 Egyptian years nearly.

If the ſun's parallax be 9″, the ſolid content of the earth is $\frac{1}{1217420}$ of the ſolid content of the ſun.

Therefore, in 385130000 Egyptian years, the ſun ſhould loſe $\frac{1}{1217420}$th of his ſolid content, and conſequently in the ſame time the diameter of the ſun ſhould loſe $\frac{1}{38522600}$ th of its whole dimenſions. And in the ſame time the apparent diameter ſhould loſe the like part of its quantity, if the diſtance between the earth and ſun remained unaltered.

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[f] I ſuppoſe the ſun to be a fluid maſs: by this hypotheſis, I give the utmoſt force to Dr. Franklin's objection; for, the more perfectly the ſun is fluid, the more ſuddenly will the vacuities be filled up.

[g] Vide Inſtant. Product. p. 30.

[h] Ibid. p. 16.

The ſun's mean apparent diameter contains 1922 ſeconds. Therefore $\frac{1}{38522600}$ th of the ⊙'s apparent diameter, is $\frac{1}{1900}$th of one ſecond very nearly. So inconſiderable would be the whole diminution of the ſun's apparent diameter, that could ariſe from the waſte of his ſubſtance, in 385130000 Egyptian years.

But the waſte of the ſun's ſubſtance muſt leſſen the attraction between the earth and ſun. As the attraction leſſens, the earth will recede to greater diſtances. And hence there will ariſe a further diminution of the ſun's apparent diameter, and a prolongation of the anomaliſtic year.

The denſity of each particle of light has been ſuppoſed three times that of iron, or 23 times the mean denſity of the earth[i]. Therefore, as often as the ſun's loſs by light amounts to an earth in ſize, it will amount to 23 earths in matter.

The matter of 23 earths is $\frac{1}{13232}$d of the ſun's matter, if the ſun's parallax be 9″.

Therefore in 385130000 Egyptian years the ſun loſes $\frac{1}{13232}$d of his matter; and the gravitation towards the ſun, at any given diſtance, diminiſhes in the ſame proportion. But this alteration is much too ſmall to diſcover itſelf in the motions of the earth or any of the planets. I will not at preſent conſider, by what law the diſtance of the planets from the ſun would increaſe, becauſe the enquiry could not be reduced to a ſmall compaſs; but it is obvious, that, whatever that law may be, it muſt ariſe ſolely from the diminution of gravitation: and the like is to be ſaid of the increaſe of the anomaliſtic periods. And therefore, while the diminution of gravitation is inſenſible, the changes in theſe circumſtances muſt be inſenſible too. Of all the changes to which our ſyſtem may be obnoxious, thoſe which ſhould ariſe from the waſte of the ſun's ſubſtance in light, upon the ſuppoſition that light is an actual emanation of matter from the ſun, reckoning that waſte at the utmoſt, are perhaps the leaſt conſiderable.

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[i] I reckon the mean denſity of the earth no greater than that of common water. It is certain that it cannot be leſs. Sir Iſaac Newton reckons it 5 or 6 times greater; but I confeſs that I am not ſatisfied with his reaſons for making it ſo great. If I have under-rated it, I have, in ſo doing, given the advantage to Dr. Franklin's objection.

In the foregoing computations, the inſtantaneous emiſſion of light has been greatly over-rated. For if the particles of light were of the magnitude and denſity which has been aſſigned to each, and were to iſſue from the ſun in the cloſe arrangement that has been ſuppoſed, they would form a ſort of cruſt upon the ſun's ſurface, at leaſt 12 times more denſe than water, *i. e.* 9600 times more denſe than our atmoſphere in the parts next the earth's ſurface, if the denſity of common water compared to that of air be reckoned only as 800 to 1 [k]. But if the denſity of light upon the ſun's ſurface be 9600 that of our air, its denſity when it arrives at the earth, or its denſity upon the ſurface of the orbis magnus[1], ſhould be more than $\frac{1}{5}$th part of the denſity of our air. When ſubſtances of different ſpecific gravities, as a piece of gold and a piece of cork, deſcend, in the exhauſted receiver, with equal velocities, and fall equal heights in the ſame time, it is obvious, that the denſity of the medium, through which they fall, bears no ſenſible proportion to the denſity even of the lighter ſubſtance. The medium through which ſuch ſubſtances fall, in a tranſparent glaſs receiver, is compoſed of ſome ſmall portion of rarified air, and, to all appearance, of the ſame quantity of light, as the receiver contains before exhauſtion. For the quantity of light, in a tranſparent receiver, can by no means ſuffer any diminution, by the action of the air-pump. The denſity of the light therefore, in our air, is certainly too ſmall to bear any ſenſible proportion to that of gold or even of cork. And the denſity of cork bears, though a great, yet a finite and a ſenſible proportion to that of the atmoſphere; becauſe in air gold and cork do not deſcend with equal velocities. Hence, I think, I may conclude with the greateſt certainty, that the denſity of the ſun's light at the earth, or upon the ſurface of the orbis magnus, is too ſmall to bear any ſenſible proportion to the denſity of common air; and I ſhall hardly underrate the denſity of the light, if I reckon it only $\frac{1}{10000}$th part of the denſity of the air, or $\frac{1}{8000000}$th part of the denſity of common water. Suppoſe this to be the denſity of light upon the ſurface of the orbis magnus, and it will be found by computation, that its denſity upon the ſun's ſurface, muſt be leſs than $\frac{1}{173}$d of the denſity of common water.

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[k] It is well known that the denſity of water to that of air, is as 850 to 1 at leaſt.

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[1] By the ſurface of the orbis magnus, I mean to denote a particular place in abſolute ſpace, namely the ſurface of that ſphere which is concentric with the ☉, and hath the earth's mean diſtance from the ☉ for its ſemidiameter.

From theſe conſiderations, I think it may be concluded with the greateſt certainty, that the quantity of matter that iſſues from the ſun in light, has been greatly over-rated in the foregoing computations.

I apprehend, however, that the denſity of each ſeparate particle cannot be leſs than has been ſuppoſed: but that the magnitude of each is leſs, and the arrangement leſs compact[m]. Let the denſity of each, and the number that iſſues at any one time from the ſun, remain as before; and let us conſider, in what proportion the magnitude of each particle, muſt be diminiſhed, ſo that they may altogether form a fluid, on the ſun's ſurface, 173 times leſs denſe than water. Let ABCD repreſent a ſection of the ſun's ſphere, through the centre S[n]. In the ſpherical ſurface ABCD, take any point B. Join SB, and take Be, Bf, each equal to the ſemidiameter of a particle of light. Upon the centre S, at the diſtances Se, Sf, imagine two other ſpheres, egh, fkl, one incloſing, the other incloſed within the ſphere ABCD. The ſolid ſpace $efklgh$, is the ſpace that contains all the particles of light (with their interſtices) that iſſue together from the ſpherical ſurface ABCD; and becauſe ef bears an exceeding ſmall proportion to SB, therefore the ſpherical ſurfaces egh, fkl, are very nearly equal each to the other, and to the ſpherical ſurface ABCD; and the ſolid ſpace contained between them is very nearly equal to a cylinder, upon a baſe equal to the ſpherical ſurface ABCD, and of altitude equal to ef. Therefore diminiſh the diameter of each particle of light, that is, diminiſh ef, in any proportion whatſoever, and the ſolid ſpace ABCD $\times ef$ diminiſhes in the ſame proportion. And if the matter in that ſpace were given, the denſity of it would always be as $\frac{1}{ef}$. But the matter of each particle of light, and conſequently the matter of the given number in the ſpace ABCD $\times ef$, is always as ef^3. Therefore the denſity of the ſubſtance formed by light in the ſpace ABCD $\times ef$, is always as $\frac{ef^3}{ef}$, or as ef^2.

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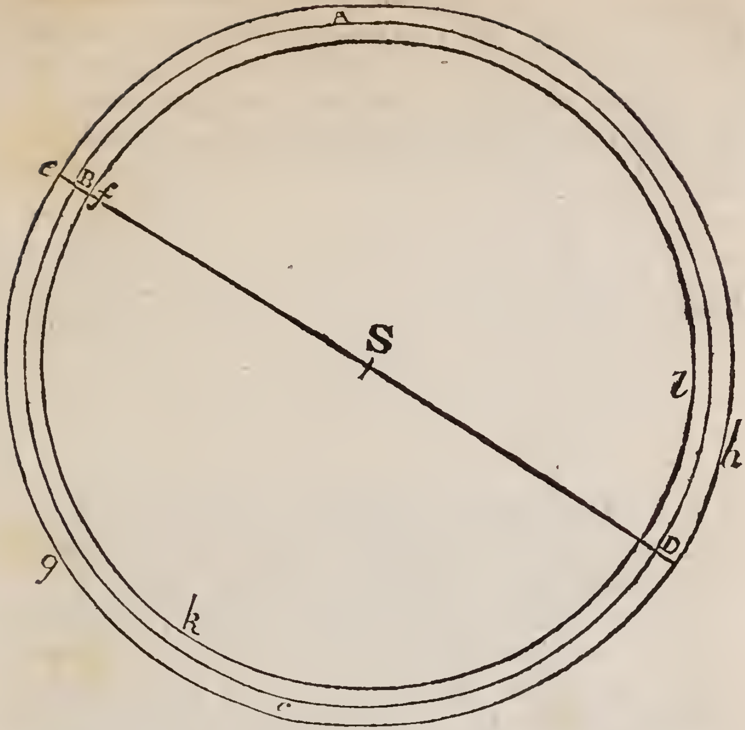
[m] Vide Inſtant. Product. p. 37.

[n] See the fig. p. 429.

That is, the number of ſpherical particles of light, in the ſolid ſpace ABCD $\times ef$, being given, and the denſity of each particle, and the ſpherical ſurface ABCD being given, the ſquare of the diameter of each particle will be as the denſity of the ſubſtance they compoſe, upon the ſurface of the ſun. But it has been found, that if the diameter of each ſpherule were one millionth of one millionth of an inch, the greateſt number of ſuch ſpherules, that the ſun's ſurface can contain at once, would, with the denſity which has been aſſigned to each ſeparate particle, form a ſubſtance upon it 12 times more denſe than water. Therefore, that the ſame number of ſuch particles ſhould form a ſubſtance about 173 times leſs denſe than water, the diameter of each muſt not exceed $\frac{10}{456}$th of one millionth of one millionth of an inch. But I have ſhewn that the

greateſt



greateſt probable denſity of light upon the ſun's ſurface does not amount to 173d part of the denſity of common water. Therefore the diameter of each ſpherule does not exceed $\frac{10}{456}$th, or more accurately, $\frac{1000}{45648}$th, of one millionth of one millionth of an inch; and is probably ſtill leſs [n]. More than 95100 ſpherules of this ſize go to make up one of the ſize firſt aſſumed. Therefore, though the ſun ſhould loſe as much matter, as can be ſuppoſed to be contained in any ſingle emiſſion of light, 95100 times in every ſecond, no ſenſible alterations in the ſyſtem could take place in millions of years. And perhaps light does not iſſue from the ſun ſo frequently as 95100 times in a ſecond.

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[n] Inſtant. Product. p. 38—41.

June 13, 1770.

S. Horſley.

P. S. The late Dr. Pemberton, of Greſham College, to whom the foregoing paper was communicated, about twelve months before his death, in a letter with which he favoured the authour, upon the ſubject, remarked, that he had no material objection to any part of the reaſoning, except it was, that the particles of light are too peremptorily ſpoken of as ſpherules. Their real figure is quite unknown; but the probability is, that they are not ſpherical, ſince Sir Iſaac Newton found that their different ſides have different properties. As the like objection may occur to others, it may not be improper to add a few words to obviate it. I would by no means be underſtood to aſſert, that the particles of light are of a ſpherical figure. But, whatever their *figure* may be, I conceive that their *ſize* muſt be ſo ſmall, that the diagonal of each little ſolid cannot exceed one millionth of one millionth of an inch, or, at leaſt, that each is capable of being circumſcribed within a ſphere of that diameter. To the reaſons which are given for making them ſo ſmall, in the treatiſe ſo often referred to, I ſhall here add another, which may perhaps be more generally convincing :—namely, that theſe bodies muſt be ſo minute as to find room to enter in, in ſwarms, at the pupils of the eyes of the ſmalleſt microſcopic animals, and not to injure, by their ſtroke, the very delicate fibres of their optic nerves, nor to lacerate the edges of the uvea; which thoſe that enter near the ſides of the perforation, if their figure be not round, muſt often bruſh with their angles, as they paſs by. Now, if it be granted, that the greateſt diagonal of each ſolid corpuſcle of light does not exceed one millionth of one millionth of an inch, or that each is capable of being circumſcribed with a ſphere of that diameter; then, the ſolid content of a ſphere of that diameter is the *maximum* of the ſolid content of each corpuſcle, and the matter in ſuch a ſphere, of due denſity, is the maximum of the matter in each corpuſcle. The maximum therefore of the force of motion in each particle, is the force of a ſpherule of the ſize aſſumed, moving with the known velocity of light; and therefore, be the figure what it will, my concluſion (p.420.) which reſts entirely on the maximum of ſize and matter, will ſtill hold good, unleſs it can be ſhewn that I have under-rated the denſity of each particle; and even if it could be proved that I have aſſumed the denſity too ſmall, in the proportion of 1 to 12 thouſand times the ſquare of one million, ſtill the general concluſion will not be ſhaken: for this vaſt increaſe of the denſity will raiſe the force of motion, in each corpuſcle, to no more than that of an iron ball $\frac{1}{4}$ of an inch diameter, moving one inch in a whole year.

Again, in the diagram, p. 429, S B being the diameter of the ſun, and *f* B, B *e* being each the half of one millionth of one millionth of an inch, the ſpace contained between the ſpherical ſurfaces *f k l*, *e g h*, is the maximum of the ſpace that the particles of light with their due proportion of interſtice can fill, as they ſtart forth from the ſurface of the ſun. For, whatever their figure be, it muſt be ſuch a one as can be laid between theſe two ſpherical ſurfaces. Now the quantity of matter in this ſpace, muſt not be greater, than it would be upon the hypotheſis that each figure was ſpherical, and the number of ſpherical particles the greateſt poſſible. Since, upon that hypotheſis, the denſity of matter, crowded into this ſpace, is vaſtly too great, to be conſiſtent with the appearances of nature (vide p. 425, 426.); and conſequently a greater denſity would be more inconſiſtent. Therefore the maximum of the matter, and conſequently, if the denſity of each ſeparate particle has been rightly aſſumed, the maximum of the ſolid content, in each emiſſion of light, is what I have made it; at leaſt it does not exceed what I have made it, be the figure of the particles what it will; and my concluſions (p. 424 and 425.) which reſt entirely upon the maximum of the ſolid content, and that of the matter, and are the ſtronger the leſs theſe maxima be, will ſtill hold good. But if theſe concluſions ſtand, the objections moved by Dr. Franklin vaniſh.

The only part of my reaſoning which will be affected, by ſuppoſing the figure of the particles of light not to be ſpherical, will be that, (p. 427, &c.) in which I attempt to ſhew, in what proportion the magnitude of each particle, and the matter, contained in each emiſſion, muſt be leſs than the maximum, in order to make the denſity of light no greater, than may be conſiſtent with the appearances of nature. Here indeed the figure of the corpuſcles is of great importance. The diminution neceſſary will be very different in different figures, and the figure, I confeſs, may be ſuch, as to make it much leſs, than what I have ſhewn to be requiſite upon the ſpherical hypotheſis. However, if $\frac{1}{10000}$th part of the denſity of our air be admitted to be as great a denſity, as can reaſonably be allowed to light, at the ſurface of the orbis magnus, the matter of each emiſſion muſt not, upon any hypotheſis of the figure of the corpuſcles, exceed $\frac{1}{2084}$th of the maximum.

For, in order to bring the denſity down, from the maximum to any other given limit, either the matter muſt remain unaltered, and the ſpace, which it is ſuppoſed to occupy, be encreaſed, in the proportion in which the denſity is to be diminiſhed; or the ſpace muſt remain unaltered, and the matter be diminiſhed in proportion to the denſity; or, if both ſpace and matter be altered, the matter muſt be changed in the proportion compounded of the two, in which both ſpace and denſity are varied. Now the ſpace which we ſuppoſe the light to occupy, as it is emerging from the ſurface of the ſun, muſt not exceed the ſpace contained between the ſpherical ſurfaces *e g h*, *f k l* (fig. p. 429.). For that ſpace, as has been obſerved, is the maximum. This ſpace may be diminiſhed; but if it be diminiſhed, the matter being diminiſhed as the ſpace and as the denſity jointly, muſt be more diminiſhed than in the ſimple proportion of the denſity. Therefore the diminution of the matter will be the leaſt poſſible, if, the ſpace being ſuppoſed to continue at its maximum, the matter be diminiſhed in the ſimple proportion of the maximum of the denſity to the denſity required. Various formations of the particles of light might be thought of which would anſwer this purpoſe. It might be anſwered, indeed, upon the ſpherical hypotheſis, by diminiſhing the number of ſpherules, retaining the maximum of their ſize; or retaining the maximum both of ſize and number, if it can be thought reaſonable to diminiſh the denſity of each particle. But, upon any hypotheſis, the diminution of matter cannot be leſs than has been ſaid. That is, the matter of each emiſſion cannot poſſibly exceed $\frac{1}{2084}$th of the maximum. For the proportion of the maximum of the denſity to the denſity required, will be found by computation, to be that of 2084 to 1, very nearly. Therefore the utmoſt probable amount of one emiſſion is $\frac{1}{2084}$th of the maximum, be the figure of the corpuſcles what it will. And the ſun may loſe the quantity of a whole emiſſion 2084 times in a ſecond without any ſenſible conſequences.

I cannot apprehend, from any quarter, ſo unphiloſophical an objection, as that the extreme minuteneſs of the particles of light, which I have ſhewn to be neceſſary, if light be really matter, is an argument againſt their exiſtence. Size is a meer accident, and no part of the eſſence of any being. Great and ſmall, applied to finite things, are purely terms of compariſon. One Being only is abſolutely Great: he whoſe ſubſtance pervades and fills the whole and every part of Abſolute Space; becauſe, in reſpect of Him, all things that are are little.

Notwithſtanding the maximum of moving force, in each particle of light is ſo inconſiderable as I have ſhewn it to be, yet the number of particles, out of each emiſſion, which are directed towards the earth, and fall upon its enlightened hemiſphere, being exceeding great, it may perhaps be imagined, that the force impreſſed by them all upon the earth, if they all actually ſtrike its ſurface, may amount to ſomething worth attending to. I have taken the pains to ſatisfy myſelf upon this queſtion, and ſhall briefly mention the reſult of my computations.

Reckoning every emiſſion at its maximum, I find that the number of the particles, out of each, which ſhould fall upon the earth's enlightened hemiſphere, is 492023xxx1, or that of which the logarithm is 36.6919854. The moving forces of this whole number amount to as much, as the force of an iron ball, of one yard diameter, flying 68 mile and 887 yards in one ſecond. But the progreſſive force, which they might communicate to the earth, does not exceed that of an iron ball of the ſame ſize, flying 34 mile and 443 yards in one ſecond[°]. And, if this whole force be transferred

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[°] Only one particle out of each emiſſion from any very ſmall given part of the ſun's ſurface, can ſtrike the earth's ſurface per-from the iron ball to the globe of the earth, the progreſſive velocity of the earth's center will be found to be no more, than that with which a body would traverſe about $\frac{1}{2}$ of one millionth of one millionth of

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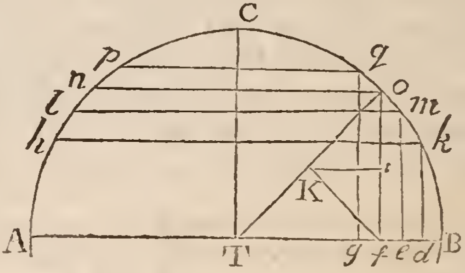
pendicularly. The force of every particle, which impinges obliquely, is reſolvable into two, one perpendicular to the ſurface, and the other parallel to it. That part of the force, which is perpendicular to the earth's ſurface, produces a progreſſive motion of the whole globe. The other, which is parallel to the ſurface, contributes nothing to the progreſſive motion, but tends to produce a rotation of the globe upon an axis. Hence the progreſſive force of motion, communicated to the globe, is leſs than it would be, if all the particles ſtruck the ſurface perpendicularly. It is further leſſened upon another account, namely, that the whole of the perpendicular force is not effective in moving the earths center. I find by computation that the diminution upon the whole is $\frac{1}{2}$. For the eaſe of the mathematical reader, I ſhall briefly ſtate the principles upon which this computation of the force with which the earth may be ſtruck by light, has been framed.

The number of particles, which are directed to the earth out of each emiſſion, is to half the number of the whole emiſſion, in the duplicate proportion of the chord of the ſun's horizontal parallax, to the chord of 90°: by the known doctrine of Archimedes. Hence, the number of the whole emiſſion being determined, the number of thoſe which tend towards the earth, is given. And the force of each ſingle particle being given, the ſum of the forces of that given number is given; and this is the progreſſive force which would be impreſſed upon the earth, if all the force of each particle were effective. In what proportion the progreſſive force is diminiſhed, on account of the various obliquities of impulſe, is thus inveſtigated.



Imagine T to be the earth's center and ACB to be half a great circle an inch in 100″. This is the utmoſt effect of the force impreſſed upon the earth by each emiſſion. If the emiſſions were inceſſant, this might be conſidered as a central force, counteracting the ſun's attraction:

of the earth, perpendicular to that which ſeparates the enlightened and the dark hemiſphere, and which ſhall be called the terminator. Upon the plane of this ſemicircle, ſuppoſe the terminator, and its parallels in the enlightened hemiſphere, to be projected, into right lines AB, *kh*, *lm*, *no*, &c. which are diameters of the circles reſpectively, of which they are the projections. The ſun's diſtance may be conſidered as infinite; and therefore the rays of light, i. e. the directions of the particles, when they reach the earth's ſurface, are to be conſidered as parallel to each other, and all of them perpendicular to the plane of the terminator. Now imagine the whole enlightened hemiſphere to be divided into innumerable little zones AB*kh*, *hkml*, *mlno*, &c. by ſmall circles parallel to the terminator. Let the breadths of theſe little zones, meaſured upon a great circle paſſing through the poles of the terminator, that is, let the infiniteſimal arcs *Bk*, *km*, *no*, &c. be ſo proportioned to each other, that perpendiculars *kd*, *me*, *of*, &c. being drawn from the extremities of theſe arcs, to the right line AB, which is the common interſection of the great circle ACB, and the plane of the terminator, the infiniteſimal ſegments of that line B*d*, *de*, *ef*, &c. may be equal. Now imagine the particles of light which fall upon any one of theſe little zones, for inſtance, *noqp*, to meet with no reſiſtance from the earth's ſurface, but to penetrate the globe, and to paſs on without refraction or inflection, in the direction perpendicular to the terminator, till they arrive at the plane of the terminator, and there ſuppoſe them to ſtop, and each to lye ſtill, in the place on which it falls. It is evident that the particles of light that fall upon, and have been ſuppoſed to paſs through, the ſpherical zone *pqon*, will, with their proper interſtices, cover that annular ſpace upon the plane of the terminator, which is the orthographical projection of the zone *pqon*, upon that plane, and is compriſed between the circumferences of circles, of which the right lines T*g* and T*f* are the radii. Hence the number of the particles of light, which fall upon the evaneſcent zone *pqon*, are as that evaneſcent annular ſpace for its tendency is to puſh the earth directly from the ſun. I need not ſay, that it is infinitely too ſmall, in compariſon of the ſun's attraction, to produce any ſenſible effect.

which they cover, that is, as $gf \times$ the circumference of the circle of which Tf is the radius, that is, as $gf \times$ in the right line Tf. But that part of the force of each particle, impinging on the zone $pqon$, which is perpendicular to the ſurface of the zone, is as of, if TB (the ſemidiameter of the earth) be put for the whole force. For join To, and draw fK perpendicular to To. The particle impinging at o moves in the direction of. Let the right line of then expreſs its whole force, and this force of is compoſed of the two oK, Kf, of which oK is perpendicular to the ſurface of the ſphere at o, and Kf is parallel to it. But $oK : of = of : oT$ or TB. Again, through K, draw Kt perpendicular to of. The force oK is reſolved into two ot, tk, of which ot is perpendicular to the plane of the terminator, and is the only part of the force oK, which tends to produce a progreſſive motion of the globe, in the direction of the impinging particles, that is, directly from the ſun. The other part tK urges the center of the globe along the plane of the terminator; but the forces tK being equal and contrary on oppoſite ſides of, and at equal diſtances from, the perpendicular ray, deſtroy each other's effects. Now it has been ſhewn, that the whole force of the particle impinging at o is to that part of its force which is perpendicular to the earth's ſurface at o, as TB to of. And it is manifeſt that oK is to ot, that is, the perpendicular force, of the particle impinging at o, is to that part of it, which is effective in moving the earth's center, as TB to of. Therefore the whole force of the particle impinging at o is to its effective part, as TB^2 to of^2. That is, the effective part is as of^2. The number of particles impinging on the zone, has been ſhewn to be as $gf \times Tf$. The progreſſive force of motion excited in the earth's center, by all the particles impinging on the infiniteſſimal zone $pqon$, muſt be, as the number of the particles and the effective part of each, jointly; that is, as $gf \times Tf \times of^2$, or writing a for TB, and x for Bf, as $\dot{x} \times \overline{a-x} \times \overline{2ax-x^2}$. And this is the fluxion of the progreſſive force of motion excited in the globe, by the particles impinging upon that ſegment of the

The rotatory forces mentioned in the laſt note if they were infinitely greater than they really are, would not, in the leaſt degree, diſturb the diurnal rotation; becauſe every one of them is deſtroyed, by an equal one, in a contrary direction, on the other ſide of, and at an equal diſtance from, the perpendicular ray.

I have enquired, what may be the utmoſt ſtroke, which the retina of a common eye ſuſtains, when the eye, in a bright day, is turned up directly to the ſun. This force will evidently be at its maximum, if the emiſſion be reckoned at its maximum. The number of particles which enter an eye, looking up directly at the ſun, are to the number out of each emiſſion, which are directed towards the earth, in the duplicate proportion of the diameter of the pupil to the diameter of the earth. And the force with which the eye is ſtruck, is to the ſum of the forces of all the particles which ſtrike the earth, in the ſame proportion. If, therefore, the diameter of the pupil, when the eye is expoſed to the direct impulſe of the ſun's rays, be reckoned $\frac{1}{10}$th of an inch, which I apprehend muſt rather exceed than

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ſphere, of which $pABq$ is the projection. The number of particles impinging on the zone $pqon$, being as $gf \times Tf$, or as $\dot{x} \times \overline{a-x}$, if each impinged perpendicularly, and its whole force were effective, the ſum of the effective forces impreſſed upon the whole, would be as $gf \times Tf \times TB^2$, or $\dot{x} \times \overline{a^3-a^2x}$. And this would be the fluxion of the progreſſive force of motion of the globe excited by the particles impinging upon the ſegment of which $pABq$ is the projection, if all impinged in directions perpendicular to the ſurface, and the whole of their forces were effective. The fluent of $\dot{x} \times \overline{a-x} \times \overline{2ax-x^2}$ is $\frac{1}{4} \times \overline{2ax-x^2}|^{\frac{3}{2}}$. And the fluent of $\dot{x} \times \overline{a^3-a^2x}$ is $a^3x - \frac{1}{2}a^2x^2$.

than fall ſhort of its real magnitude, in thoſe circumſtances, it will be found that every ſtroke which it receives from them, exceeds not that, which an iron ſhot, $\frac{1}{4}$ of an inch diameter, would give, moving only at the rate of 16,16 inches in a year. This would be the ſtroke if the emiſſion were at its maximum. Is it not owing to the extreme minuteneſs of the fibres of the nerves, that a ſtroke, which is certainly leſs than the $\frac{1}{2084}$th part of this, is not ſuſtained by our organs, without pain ?

When $x=a$, the firſt of theſe two fluents is the ſum of the progreſſive forces actually impreſſed upon the whole hemiſphere ACB, and the latter is the ſum of the forces which would be ſo impreſſed, if all the impinging particles impinged perpendicularly, and the whole force of each were effective. But when $x=a$ the firſt fluent becomes $\frac{1}{4}\,a^4$. And the latter becomes $\frac{1}{2}\,a^4$. Whence it is manifeſt that the progreſſive motion communicated to the globe of the earth, by the particles of light, is to the force which they would communicate, if the whole force of each were effective, in the proportion before aſſigned, of one to two.

Received May 14, 1770.

XXXVI. *Some New Theorems for computing the Areas of certain Curve Lines: By Mr.* John Landen, *F.R.S.*

Read June 28, 1770.

THE learned editor of Mr. Cotes's *Harmonia Menſurarum* firſt gave us, in that book, the celebrated theorems for computing the areas of the curves whoſe ordinates are expreſſed by $\frac{x^p}{a^n+x^n}$, $\frac{x^p}{\overline{a^n+x^n}\times\overline{e^n+x^n}}$, or $\frac{x^p}{a^{2n}+2ca^nx^n+x^{2n}}$; and ſeveral other writers have ſince done the like. Which theorems conſiſt of many terms, being obtained by previouſly reſolving the expreſſion for the ordinate, into others of a more ſimple form. Now I have found, that the *whole* area of every ſuch curve (when finite) may be aſſigned by theorems remarkably conciſe, without the trouble of reſolving the expreſſion for the ordinate as aforeſaid: and, as in the reſolution of problems, the whole area of a curve is more commonly wanted than a part thereof; and as theſe new theorems enable us to compute ſuch whole areas as above-mentioned, or the whole fluents of $\frac{x^p\dot{x}}{a^n+x^n}$, $\frac{x^p\dot{x}}{\overline{a^n+x^n}\times\overline{e^n+x^n}}$, and $\frac{x^p\dot{x}}{a^{2n}+2ca^nx^n+x^{2n}}$, with admirable facility; I do myſelf the honour of communicating them to the Royal Society, preſuming they may be thought worthy to be publiſhed in the Philoſophical Tranſactions.

THEOREM I.

m being any poſitive integer or fraction, and n any ſuch integer or fraction, greater than m; the *whole* area of the curve, whoſe abſciſſa is x, and ordinate $\frac{x^{m-1}}{a^n+x^n}$ is $=\frac{a^{m-n}}{fn}\times A$.

THEOREM II.

m and n being as before-mentioned, the *whole* area of the curve, whoſe abſciſſa is x, and ordinate $\frac{x^{n\pm m-1}}{\overline{a^n+x^n}\times\overline{e^n+x^n}}$ is $=\pm\frac{a^{\pm m}-e^{\pm m}}{a^n-e^n}\times\frac{A}{fn}$.

Note. When e is $=a$, the expreſſion for the area becomes $=\frac{ma^{\pm m-n}}{fn^2}\times A$.

THEOREM III.

m and n being as in the preceding theorems, the *whole* area of the curve, whoſe abſciſſa is x, and ordinate $\frac{x^{n\pm m-1}}{a^{2n}+2ca^nx^n+x^{2n}}$ is $=\frac{g\,a^{\pm m-n}}{bfn}\times A$.

Note. If m be $=o$, the area will be $=\frac{a^{-n}B}{bn}$.

In theſe theorems,

A denotes the ſemi-periphery of the circle, whoſe radius is 1;

B an arc of the ſame circle, whoſe coſine is c, and ſine b;

f the ſine of the arc $\frac{m}{n} \times A$;

g the ſine of the arc $\frac{m}{n} \times B$.

Concerning the inveſtigation of theſe theorems, it is ſufficient to ſay, they are directly obtained by the help of my new method of comparing curvilineal areas, inſerted in the Philoſ. Tranſact. for the year 1768.

It is obvious, that, by means of the above theorems, we may very readily compute the *whole* areas (when finite) of the curves, whoſe ordinates are $\frac{xp}{p+qx^n+rx^{2n}+x^{3n}}$, and $\frac{xp}{p+qx^n+rx^{2n}+sx^{3n}+x^{4n}}$, &c. ſeeing theſe expreſſions may be eaſily transformed into others ſimilar to thoſe already conſidered.

XXXVII. *Extract of Two Letters from the late Capt.* Alexander Roſe, *of the 52d Regiment, to Dr.* Murdoch, *F. R. S.*

From LETTER I.

Madras, 20 Sept. 1768.

Read July 5, 1770.

* * * * * WE arrived here the 15th of this month; at firſt ſetting out, we had bad weather, having been driven by a contrary wind upon the coaſt of Portugal. From thence we took our departure for the Canaries; where I had an opportunity of gratifying my curioſity by a ſight of the famous Pic of Teneriffe. About ten days after, we made the Cap de Verde-Iſlands; and, by a miſtake of the Captain (which I was glad had happened), ſailed cloſe by the iſland del Fuego, famous for its volcano, which we ſaw in its perfection, and which fully comes up to Virgil's deſcription of Mount Ætna. We had no ſooner loſt ſight of the Cap de Verdes, than we were overtaken with a ſtorm, which laſted a conſiderable time, and were near driven upon the coaſt of Braſil. But the ſtorm abating, and the wind coming fair for a month or more, we had a ſpeedy voyage to the Cape of Good Hope, where we were plentifully

ſupplied with every thing we could wiſh for our refreſhment, after ſo long a voyage. The climate and country there are ſo good, that the trees, fruits, and vegetables, both of the hot and the cold climates, flouriſh and thrive in the greateſt perfection. The Dutch there, are juſt the ſame kind of people they are in Holland: ſo that, for variety's ſake, I took a jaunt to the Hottentots crawl-ſhips, which I had a great curioſity to ſee. I found Kolben's account of them to be, in general, very juſt. He takes no notice, however, of their muſic, which I found to be very tolerable; their chief inſtrument being a large cocoa nut-ſhell, ſtrung with guts, and ſomewhat reſembling a guittar. I am convinced they have always had a belief of one Supreme Deity, and other ſubordinate ones, as alſo of a future ſtate.

We found, by exact and repeated obſervations, that the variation of the needle, at the Cape, was 19°½; though, in the lateſt Variation-chart, it is laid down at 18°.

We left that place the 20th of July; and during our long voyage to India, I frequently amuſed myſelf with microſcopical obſervations, of which I ſhall give you ſome account; becauſe, for what I know, ſome of them may not have fallen under the notice of the writers on this ſubject, who make no mention of that ſpecies of *animalcula*, which ſwarm in the atmoſphere of the immenſe ocean, their proper element.

As I could find no great variety of objects on board the ſhip, I endeavoured to produce *animalcula*, by the common methods (by pepper-water, hay, &c.);

&c.); but finding they did not anſwer my purpoſe, I tried freſh-water alone, which I expoſed to the open air for two days, and upon examining found a good number of *animalcula*; but they ſoon languiſhed and died. I afterwards expoſed the freſh water for two hours only, and found ſome in it, which lived but a very ſhort time. I therefore reſolved to try ſea-water, which, having been expoſed to the open air, in a very ſhort time, ſwarmed with *animalcula*; theſe, upon the ſtricteſt examination, I found to be exactly of the ſame ſpecies with thoſe of freſh water: only they ſeemed much more active and lively, and inſtead of dying in a ſhort time, as thoſe of the freſh water, they continued to increaſe and thrive wonderfully, the longer I kept them. I then removed ſome of them out of the ſalt into the freſh water; but they ſoon languiſhed and died. Whence I conclude them to be a quite different ſpecies from what we find upon, or near, the land. I began my experiments about ten days after we loſt ſight of land, and continued them the whole voyage; ſo that, with the help of ſome books, I paſſed my time very agreeably.

I had provided myſelf with a thermometer, to meaſure the degrees of heat and cold, in the different climates we were to paſs through; and found the mercury to riſe from 55 to 80° of Fahrenheit's ſcale: which laſt it never exceeded, though we were ſometimes becalmed between the tropics, and upon the line. This degree of heat I have often experienced in Canada, as I dare ſay you ſometimes have in England.

When I have any thing worth communicating from this part of the world, I ſhall not fail to acquaint you with it.

From LETTER II.

Muxadabad, in Bengal,
20 Auguſt, 1769.

SOON after my arrival in Bengal, I was appointed, by the governor, ſurveyor of the company's lands, and ordered to ſurvey the provinces to the North and North Eaſt, as far as Napal; for which I ſet out in November laſt, accompanied with a military force; as there was reaſon to believe the mountaineers would obſtruct the ſurvey. Having finiſhed this work, I diſmiſſed the troops; and deſired the Raja of Napal would give me leave to viſit his country, attended only by a few ſervants; which he with ſome difficulty granted.

The country of Napal is a large flat, ſurrounded with three ranges of almoſt inacceſſible mountains, which are covered with ſnow all the winter, and a great part of the ſummer: ſo that, when every thing on the plains of India is almoſt burnt up with exceſſive heat, the natives of thoſe mountains enjoy a cool temperate air. They are a very different ſpecies of people from the inhabitants of the plains. Their complexions are olive, their features broad and flat;

and their perſons ſhort and ſtrongly made: they profeſs the Gentoo religion; but differ from them, in that they ſacrifice, and eat moſt kinds of fleſh, excepting that of the cow and of the hog. Their language is the Nagri, which I have good reaſon to believe was the original language of India, and is more ancient than even the Shanſcritta; which I imagine to have been formed and invented by the Bramins, in order to veil their religion from the vulgar. There are at preſent two dialects of the Nagri, one of which is not very common, and is called the Bengal; but this is underſtood by only a very few, and may be called the ancient Nagri; and the other the modern. I found many manuſcripts among the mountaineers, ſome of which treat of the hiſtory of India, going back above three thouſand years. And I am convinced, that, to come at the true hiſtory of the early times of this country, recourſe muſt be had to the books written in this language. I am now endeavouring to get ſome of them tranſlated.

This country has never been viſited by any Europeans, but a few Italian miſſionaries who have lately been driven out of it. I happened to meet with them by accident, and flattered myſelf I ſhould receive ſome uſeful information from them; but was much diſappointed, for they were a ſet of the moſt ignorant fellows I ever met with. Their ſuperior, who ſeemed the moſt intelligent, could give me no account of any place or thing beyond the city he lived in, although he had been twelve years in that country. He told me, however, by way of ſhewing his miſſionary zeal, that he had burnt three thouſand manuſcripts, during his ſtay there. He had two miſerable families along with him, which he called his converts.

I was greatly ſurpriſed to find many Chineſe commodities among the inhabitants of Napal; and upon enquiry found that they now have, and, for ages paſt, have had, a communication with China, by the way of Tibet. In conſequence of this information, I have given in propoſals to the governor and council here, to trace and explore this route, and am now on my way to Calcutta upon this buſineſs. The rainy ſeaſon coming faſt on, I could make but a very ſhort ſtay in Napal: though, in my opinion, it is one of the fineſt countries I ever ſaw. If my propoſals to the goverment here are agreed to, and it ſhall pleaſe God to continue my life and health*, I hope to be able to give you an account of this, and many other countries through which I muſt paſs before I reach China.

The birds and beaſts I ſaw in the Napal mountains were—Of birds, the *mumal*, a kind of pheaſant, the feathers of a fine dark brown, with red ſpots, and a red tuft upon the head—The *dcphia*, of the peacock kind; but the comb and plumage of the neck far ſurpaſs the peacock's; the tail is ſhort, and of a dirty orange colour, and ſeems a contraſt to the other plumage.—Of beaſts, the ſheep with four horns,

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* Inſtead of this, the very next India ſhip brought the melancholy news of his death: a loſs! which his family and friends muſt ever deplore—and which may even be reckoned a loſs to the ſciences, and to the public, as well as to the company, in whoſe ſervice he died: as he had, ſo early in life, to the activity and ſpirit of the brave officer, united the unbounded curioſity and cool patience of the philoſopher.

and

and a kind of deer about the ſize of a lap-dog: I ſent ſome for the governor, but they died by the way.

I am now to acquaint you, that, having procured a teleſcope and ſtop-watch, I made my obſervations upon the tranſit of Venus, which happened here on the 4th day of June, 1769.

Pheſabad, lat. 25° 30′ north.

	h	′	″	
Obſerved the planet a good way advanced on the ſun's body at	5	35	57	(app. time)
Firſt contact at the egreſs	6	52	25	
Laſt contact	7	10	47	
* Time between the firſt and laſt contacts	0	18	22	

* Whence the planet's centre was on the ſun's limb at 7^h 1′ 36″; and this compared with an obſervation of the central egreſs or ingreſs made at a diſtant place will give the ſun's parallax; the other neceſſary elements of the calculus being well eſtabliſhed. In the mean time we ſee, from the Connoiſſance des Tems for 1769, that Pheſabad in Bengal, where Captain Roſe obſerved, is 81° 45′ eaſt of Paris.

The watch had been regulated the preceding day, by equal altitudes of the ſun; the ſun's altitudes, at the two contacts, are likewiſe marked in the Captain's letter; but this part of the work he had probably entruſted to a leſs ſkillful obſerver, while his own attention was engroſſed by the teleſcope and the watch; as I find the difference of the times correſpondent, to thoſe altitudes, does not agree with the interval of the contacts; for which reaſon they are here omitted.

XXXVIII. *Ex-*

XXXVIII. *Extract of a Letter from Mr.* John Latham, *Surgeon and Midwife, at* Dartford, *in* Kent, *to Mr.* Warner, *Fellow of the Royal Society, and Senior Surgeon to* Guy's Hoſpital. *Communicated to the Royal Society by Mr.* Warner.

Read Nov. 7, 1770.

MR. A. B. about 55 years of age, was an healthy man till about 20 years ſince, when he was firſt ſeized with a fever; at which time he followed the trade of a miller, and maker of French barley. This laſt buſineſs, he tells me, is attended with very great heat to the operator, and expoſes him to a continual cloud of duſt. As ſoon as he began to work, his breath grew oppreſſed with a ſenſation of his body being puffed up all over; from which ſymptoms he was relieved by occaſionally leaving off his buſineſs. On the firſt cold caught after his entering on this kind of employment, a fever attacked him; which has generally returned ſometimes once, and ſometimes twice in a year, chiefly in autumn; but ſometimes in ſpring likewiſe: though he once miſſed being ill for two years together. After carrying on this trade for four or five years, he left it off; as he attributed his diſorder chiefly to the effects of the meal duſt. Be that as it will, the fevers have not been ſo violent ſince, as while he followed that occupation: though the cuticle, or outer ſkin, has come off, the ſame as before. As to the particulars of his illneſs, they are nearly as follow: The diſorder begins with a violent fever, attended with pains in the head, back, and limbs, accompanied with continual retchings; he ſometimes vomited up much bile, at other times little or none; the ſkin was dry, the tongue much furred, together with great thirſt, coſtiveneſs, and the urine highly coloured. At the beginning of the fever he was generally let blood; this evacuation afforded ſome relief, and by keeping his body open, and taking cooling medicines, the retchings abated in about five or ſix days: the whole ſurface of the body became yellow, though this circumſtance did not always happen. Afterwards it became florid, having the appearance of a raſh; on which he felt a great uneaſineſs for ſeveral days, with a numbneſs and tingling all over him: when the urine grew turned, and depoſited a thick ſediment. About the beginning of the third week from the firſt attack, the cuticle appeared elevated in many places. In eight or nine days afterwards it became ſo looſe, as to admit of being eaſily removed in large flakes. The cuticle of the hands from the wriſt to the fingers ends came off whole, bearing the reſemblance of a glove. He never was diſpoſed to ſweat in any part of his illneſs, and when ſweating was attempted by medicines, he grew worſe for it; nor was he much at eaſe till his urine depoſited a ſediment, after which he felt very little inconvenience, but from the rigidity of the ſkin. The nails of the patient, whoſe caſe you communicated to the Royal Society, are mentioned to have come off after the illneſs; I did not find that ever was the caſe in this perſon.

As to your communicating the above account to theRoyal Society, I beg that may be as you think proper: though, I ſhould ſuppoſe it would not prove unacceptable to the medical members of that learned body; as it may ſhew, that the caſe already recorded in the Philoſophical Tranſactions of laſt year is not a ſingular one. I wiſh the glove, I ſend you, was as perfect as I have ſeen ſome of them; but, as it is, I beg your acceptance of it.

With the human glove, you will receive a curioſity, which I think not inferior to the glove; to wit, a very ſmall fœtus, brought into the world at the ſame time with a live child at its full growth. The woman was delivered before I came to her: on examining the placenta, a ſubſtance appeared ſomewhat unuſual; and on waſhing it clean, I diſcovered the fœtus abovementioned. It had no viſible communication with the placenta, but was ſqueezed flat, though not in the leaſt putrid, and ſeemed ſhrivelled. I don't remember a caſe like this mentioned, except in Smellie's, vol. ii. p. 85, where he relates one from the Academy of Sciences at Paris, nearly ſimilar to this. May we not ſuppoſe the woman to have been with child of twins; and that this, dying, was not diſcharged, as was moſt likely to happen, but remained till the time of the natural birth, when they were both expelled together?

I am, very reſpectfully,
Gentlemen,
Your moſt obedient,
and very humble ſervant,
Joſeph Warner.

Hatton Garden,
Nov. 22, 1770.

XXXIX.

XXXIX. *Aſtronomical Obſervations made at* Cavan, *near* Strabane, *in the County of* Donegal, Ireland, *by Appointment of the Royal Society, by Mr.* Charles Maſon.

Read November 7, 1770.

Equal Altitudes of the Sun and Stars.

1769 April	Time per Clock of the equal Altitudes of the ☉'s limbs, and of *'s.		Zen. diſtance or points on the limb of the quadt. the nonius was ſet at.	☉'s Limb, and Stars obſerved.	Time per Clock when the ☉'s cent. and * paſſed the merid. from the mean of the Obſervations.
	h ' ''		° '		
☽ — 3	7 51 11½	1ſt wire	64 56	Arcturus	
	54 45	middle wire			
	58 15—	3d or laſt wire			
	7 30 7		59 20	Ditto.	
	33 41+				
	37 14—				
	9 20 14		72 40	Spica	
	25 36				
	31 1½				
♂ — 4	7 47 46		64 56	Arcturus	
	51 19				
	54 49+				
	8 26 43			Ditto	
	30 17½				
	33 49½	h ' ''			
	9 16 49	2 41 59	72 40	Spica	
	22 11				
	27 37+	31 11½			11 59 23,3
	9 43 35+	2 15 10	70 15	Ditto	
	49 49	8 56+			
	56 11	2 35			

Equal Altitudes of the Sun and Stars.

1769 April	Time per Clock of the equal Altitudes of the ⊙'s limbs, and of ✳'s.		Zen. diſtance or points on the limb of the quadt. the nonius was ſet at.	⊙'s Limb, and Stars obſerved.	Time per Clock when the ⊙'s cent. and ✳ paſſed the Merid. from the mean of the Obſervations.
	h ′ ″	h ′ ″	° ′		
☿—5	8 23 16½		59 20	Arcturus.	
	26 50				
	30 21½				
	9 13 23	2 38 30	72 40	Spica	
	18 47	33 7			
	24 11—	27 41			
	9 40 8+	2 11 44+	70 15	Ditto	11 55 56,5
	46 23½	5 30+			
	52 44—	1 59 9			
♄—8	9 36 6½	middle wire	70 15	Spica	
☽—10	9 11 30	2 18 16½	44 55	⊙'s upp. limb	
	Clouds	Clouds			
	21 12½	8 33			
	21 33	2 8 13½	54 55	⊙'s low. limb	11 44 33,5
	26 32½	3 14½			
	9 34 36	1 55 15	52 35	⊙'u pp. limb	
	Clouds	Clouds			
	45 31½	Clouds			
	51 38	1 38 7:	Ditto	⊙'s low. limb.	
	8 6 9+		59 20	Arcturus	
	9 44½				
	13 16				
	9 23 3	1 54 35	70 15	Spica	11 38 49,0
	29 16	Clouds			
	35 38	1 42 0			
☿—12	7 59 18½		59 20	Arcturus	
	8 2 53				
	6 25				

Equal Altitudes of the Sun and Stars.

April 1769	Time per Clock of the equal Altitude of the ☉'s Limbs, and of ✱'s.		Zen. diſtance or points on the limb of the quadt. the nonius was ſet at.	☉'s Limb, and Stars obſerved.	Time of Clock when the ☉'s cent. and ✱ paſſed the merid. from the mean of the Obſervations.
	h ′ ″	h ′ ″	° ′		
☿—12	8 49 25+ 54 46 9 0 13—	Cloudy at the time correſponding	72 40	Spica	
♃—20	7 47 1 50 50½ 54 33½	3 47 46 43 59 40 16—	62 36	☉'s upp. limb.	
	8 6 27½ 10 17½ 14 11	3 28 24 24 29 20 38½	60 00	Ditto	
	8 31 58½ 36 6 40 9—	3 2 48½ 3 58 44 54 38½	56 42	Ditto	
	44 32½	2 50 19		☉'s lower limb	
	7 35 23 38 56		59 20	Arcturus	
♀—21	7 28 23 31 58+ 35 30		59 20	Arcturus	11 1 3,4
	7 7 59 31 8 4 22½	2 7 28:: 2 35 1 57 43½	75 5	Spica	
	8 18 28½ 23 52 29 18—	1 43 39— 1 38 16	72 40	Ditto	
♄—22	8 45 48 49 48 53 44:		48 12	Arcturus	
	10 2 37 8 7— 13 44		39 25	Ditto	

Equal Altitudes of the Sun and Stars.

1769 April	Time per Clock of the equal Altitudes of the ☉'s limb, and of ✳'s.		Zen. diſtance of points on the limb of the quadt. the nonius was ſet at.	☉'s limb, and Stars obſerved.	Time per Clock when the ☉'s cent. and ✳ paſſed the merid. from the mean of the Obſervations.
	h ′ ″	h ′ ″	° ′		
♂ —25	8 35 33::		48 12	Arcturus	
	39 27				
	43 25				
	8 52 53		46 00	Ditto	
	57 2				
	9 1 11				
	9 52 17		39 25	Ditto	
	57 48—				
	10 3 25				
☿ —26	8 49 29+	2 22 11	46 00	Arcturus	
	53 39	18 1½			
	57 47½	13 53			
	9 48 57		39 25	Ditto	11 35 50,2
	54 28				
	10 0 5				
♀ —28	8		46 00	Arcturus	
	8 46 47				
	50 52::				
	9 7 51	1 50 4	43 00	Ditto	11 28 57,3
	12 26	45 29			
	16 59	40 55			
	9 42 5		39 25	Ditto	
	47 35				
	53 12				
☉ —30	12 5 9		40 51	Arcturus	*N.B.* This day I ſcrewed the bob of the pendulum 7 revolutions and 17 diviſions (of the nut) and ſet the clock at noon to nearly ſidereal time.
	10 11				
	15 19				
	12 23 11½		39 5	Ditto	
	28 52½				
	34 40::				

Equal Altitudes of the Sun and Stars.

1760 May	Time per Clock of the equal Altitude of the ⊙'s limb, and of ✱'s.		Zen. diſtance of points on the limb of the quadt. the nonius was ſet at.	⊙'s limb, and Stars obſerved.	Time per Clock when the ⊙'s cent. and ✱ paſſed the merid. from the mean of the Obſervations.
	h ′ ″	h ′ ″	° ′		
☽ — 1	12 12 9 50½ 14 57		40 51	Arcturus	
♂ — 2	12 4 26 9 28 14 32 : :		40 51	Arcturus	
	12 28 9+ 33 57		39 5	Ditto	
☿ — 3	12 4 4 9 5½ 14 11	16 7 14 16 2 11+ 15 57 5½	40 51	Arcturus	14 5 38,9
	12 27 46 33 36	15 43 32 37 43½	39 5	Arcturus	
♄ — 6	12 2 55+ 7 58 13 3½		40 51	Arcturus	
	12 20 58 26 38 32 27−		39 5	Ditto	
☿ —10	12 39 28½ 25 9 30 58	Cloudy at the time correſponding	39 5	Arcturus	
☽ —15	12 17 40 23 19 29 9	Clouds Clouds 15 33 15	39 5	Arcturus	14 1 12,0
♃ —18	11 58 30 12 3 31 8 37+	16 1 40 15 56 41 :	40 51	Arcturus	14 0 5,0
☿ —24	1 2 58 7 3 11 4½	6 54 38− 50 33+ Clouds	46 24	⊙'s upp. limb	

Equal Altitudes of the Sun and Stars.

1766 May	Time per Clock of the equal Altitudes of the ☉'s limb, and of ✱'s.		Zen. diſtance of points on the limb of the quadt. the nonius was ſet at.	☉'s limb, and Stars obſerved.	Time per Clock when the ☉'s cent. and ✱ paſſed the merid. from the mean of the Obſervations.
	h ′ ″	h ′ ″	° ′		
☿ — 24	1 21 4	Clouds			3 58 39,2
	25 23½	6 32 10½	44 10	☉'s upp. limb	
	29 40	6 27 55			
	1 34 16½	6 23 19½		☉'s low. limb	
☿ — 31	0 4 30	8 44 40			
	8 7	41 4	56 58	☉'s upp. limb	4 24 27,0
	11 39	37 30			
	15 22⅓	33 48½		☉'s low. limb	
	12 45 40	The ☉'s laſt limb ſet over a hill, at the diſtance of about 3½ or 4 miles.			
	14 46 10—				
	49 44		38 17		
	53 14+				
	15 12 47				
	16 24—	Cloudy	4 28	Ditto	
	19 56				
	15 36 15+	Clouds			
	39 51:	19 42 45	18 20	γ Draconis a ✱ 2d mag. merid. z.d. 3 19 38 ::	
	43 19½	39 16			
	49 8½	19 33 27			17 41 17,5
	52 42	29 53	16 29	Ditto	
	56 11	26 23			
June ♃ — 1	0 14 38	Cloudy at the times correſponding t teſe.			
	18 14½		55 57	☉'s upp. limb	
	21 47				
	25 30			Ditto low. limb	
	0 30 10				
	33 49½ :		53 45	☉'s upp. limb	
	37 27½				
	41 14			Ditto low. limb	

Equal Altitudes of the Sun and Stars.

1769 June	Time per Clock of the equal Altitudes of the ☉'s limb, and of *'s.		Zen. diſtance of points on the limb of the quadt. the nonius was ſet at.	☉'s limb, and Stars obſerved.	Time per Clock when the ☉'s cent. and * paſſed the merid. from the mean of the Obſervations.
	h ′ ″	h ′ ″	° ′		
♃ — 1	1 6 31½	Cloudy at the time-correſponding to theſe	48 45	☉'s upp. limb	
	10 20				
	14 8 :			Ditto low. limb	
	18 5				
	21 12½		46 48	☉'s upp. limb	
	25 9				
	29 2½				
	33 10½			Ditto low. limb	
	2 4 53½		41 23	☉'s up. limb	
	9 25				
	13 56—			Ditto low. limb	
	18 41				
	Cloudy with rain at night.				
♀ — 2	23 28 24	9 35 41 ::	62 59	☉'s upp. limb	
	31 58½	32 6½			
	35 30	28 35			
	23 39 11	Clouds		☉'s low. limb	
	23 55 11	9 8 53	59 9	☉'s upp. limb	
	58 45+	5 17			
	0 2 16½	1 46			
	0 6 0	8 58 7½		☉'s low. limb	
	0 16 40 :	8 47 22½	56 5	☉'s upp. limb	4 31 53,5
	20 15	43 45+			
	23 50	Clouds			
	27 33½	Clouds		☉'s low. limb	
	38 38½	Clouds	52 59		
	42 19	8 21 39		☉'s upp. limb	
	Clouds	8 18 5½			
	49 44			☉'s low. limb	

Equal Altitudes of the Sun and Stars.

1769 June	Time per Clock of the equal Altitudes of the ☉'s limb, and of *'s.		Zen. diſtance of points on the limb of the quadt. the nonius was ſet at.	☉'s limb, and Stars obſerved.	Time per Clock when the ☉'s cent. and * paſſed the merid. from the mean of the Obſervations.
	h ′ ″	h ′ ″	° ′		
♀ — 2	1 0 0	Cloudy			
	1ſt wire clou.	8 1 58½	49 45	☉'s upp. limb	
	1 5 48½	7 58 11			
	9 31	5 54 27			
	13 28			☉'s low. limb	
	Clo, 1ſt wire		47 00	☉'s upp. limb	
	1 26 30				
	30 25::				
	1 56 28	Cloudy at the times correſponding to theſe	42 43	☉'s upp. limb	
	Clouds				
	2 17 56½				
	22 37				
	27 18		40 13	☉'s upp. limb	
	32 17½			☉'s low. limb	
	12 54 30::	The ☉'s laſt limb ſet.			
	Cloudy all night.				
	20 25	Cloudy with rain.			
♄ — 3	0 16 47	8 54 43	56 30	☉'s upp. limb	
	20 23	Clouds			
	23 57:	8 47 35::			
	Then cloudy				
	0 31 27½	Clouds	54 25	☉'s upp. limb	
	35 6	Clouds			
	38 40½	8 32 51:			
	42 27	Clouds		☉'s low. limb	
	0 46 50½	Clouds	52 16	☉'s upp. limb	
	Clouds	Clouds			
	54 10—	8 17 18½			

Equal Altitudes of the Sun and Stars.

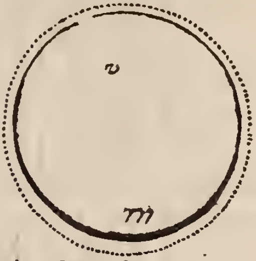
1769 June	Time per Clock of the equal Altitudes of the ☉'s limb, and of *'s.		Zen. diſtance of points on the limb of the quadt. the nonius was ſet at.	☉'s limb, and Stars obſerved.	Time per Clock when the ☉'s cent. and * paſſed the merid. from the mean of the Obſervations.
	h ′ ″	h ′ ″	° ′		
♄ — 3	0 54 19	Clouds		☉'s low. middle wire	4 35 37,8
	57 58½	Clouds		Ditto laſt wire	
	1 4 43		49 48	☉'s upp. limb	
	8 28				
	12 11½				
	16 5+			Low. limb laſt wire	
	1 22 2	Clouds	47 28	☉'s upp. limb	
	Clouds	Clouds			
	Clouds	7 41 43			
	1 33 47	7 37 40		☉'s low. limb	
	Clouds	So hazy that I could not ſee the ☉'s limb at time correſpon.	44 47	☉'s upp. limb	
	Clouds				
	1 50 46				
	55 00	7 16 25 :		☉'s low. limb	
	2 0 2 :	Clouds	42 38	☉'s upp. limb	
	Clouds	7 9			
	Clouds	7 2 54½			
	3 1 00	Clouds	36 9	☉'s upp. limb	
	6 55	6 4 25			
	13 4	Clouds			
	Then cloudy				

Though the air at external contact was not quite ſo clear as at ſome times I have ſeen, yet the ſun's limb appeared well defined, and the ſpots in the diſk very ſtrong, their edges keen and diſtinct. At the internal contact, the air was much changed, and the limb of Venus ſeemed to cohere to the Sun's limb, by a protuberance that appeared like a dark ſhade: which ſeemed to prevent my ſeeing the thread of light for about 40″ longer than I expected.

1769 June	Time per Clock of the equal Altitudes of the ☉'s limb, and of *'s.		Zen. diſtance of points of the limb of the quadt. the nonius was ſet at.	☉'s limb, and Stars obſerved.	Time per Clock when the ☉'s cent. and * paſſed the merid. from the mean of the Obſervations.
♄ — 3	h ′ ″	h ′ ″	° ′		

Equal Altitudes of the Sun and Stars.

When the planet was upon the ☉'s diſk, there appeared a faint light ſhade (having a gentle fluctuating motion) round its periphery, and wideſt on that part fartheſt on the Sun's diſk: it appeared as per fig.. the black circle repreſenting the periphery of Venus, and the dotted one that of the ſhade, which was very regular and well defined; *v* the upper, and *m* the lower part of the planet: and the whole ſhade was apparently of equal brightneſs.



	14 45 2½ 48 38 52 13 :		38 17	α Lyræ	
	15 11 39+ 15 16½ 18 48+		34 28	Ditto	
	22 15	Cloudy with rain			
	40	Cloudy			
	49 28	The eclipſe of the ſun began			
	49 35	Very plain			
	52	Cloudy with rain			
	23 28	The clouds began to break; and from this time to 23^h 54′ I endeavoured with a micrometer (of Mr. Dollond's conſtruction) to get meaſurements for determining the digits eclipſed; but was ſo interrupted by flying clouds, that nothing could be done with certainty; then cloudy with rain till the end of the eclipſe was paſt.			
☉ — 4	Cloudy				
☽ — 5	14 44 19 47 51 : : 51 23		38 17	α Lyræ	

Equal Altitudes of the Sun and Stars.

1769 June	Time per Clock of the equal Altitudes of the ☉'s limb, and of ✳'s. h ′ ″	h ′ ″	Zen. diſtance of points of the limb of the quadt. the nonius was ſet at. ° ′	☉'s limb, and Stars obſerved.	Time per Clock when the ☉'s cent. and ✳ paſſed the merid. from the mean of the Obſervations.
☽ — 5	15 10 56 14 31½ 18 5		34 28	α Lyræ	
☿ — 7	14 43 32½ 47 6 50 36		38 17	α Lyræ	
☉ — 11	14 42 4 45 37+ 49 8		38 17	α Lyræ	
♄ — 15	Wound up the clock				
☿ — 21	14 41 54 45 25—		38 17	α Lyræ	
☉ — 25	16 25 45 29 59 34 15	Cloudy at the time correſponding to theſe	23 22	α Lyræ	
♀ — 30	2 30 23 34 5— 37 43½	Clouds Clouds 9 57 7 : :	51 23	☉'s upp. limb	
	41 31			☉'s low. limb	
	2 45 29 49 14 Clouds	9 49 24 9 45 38— Clouds	49 18	☉'s upp. limb	
	14 49 35½	☉'s laſt limb ſet			6 17 28,9
	— — — 16 14 3— 18 5	Cloudy at the time correſponding	25 6	α Lyræ	
	16 23 53 28 8 32 24—		23 22	Ditto	

Equal Altitudes of the Sun and Stars.

1769 July	Time per Clock of the equal Altitudes of the ☉'s limb, and of ✱'s.		Zen. diſtance or points of the limbs of the quadt. the nonius was ſet at.	☉'s limb, and Stars obſerved.	Time per Clock when the ☉'s cent. and ✱'s paſſed the merid. from the mean of the Obſervations.
	h ′ ″	h ′ ″	° ′		
♄ — 1	3 25 22½	9 17 1	44 33	☉'s upp. limb	
	3 29 22	13 0:			
	Clouds	Clouds			
	30 40 17	Clouds	42 40	Ditto	
	Clouds	8 57 51			
	48 37	Clouds			
	16 9 39½		25 6	α Lyræ	
	Clouds				
	Clouds				
☽ — 3	16 8 53	Cloudy at the time correſponding.	25 6	α Lyræ	
	12 54½				
	16 55				
	22 45—		23 22	Ditto	
	27 0—				
	31 14½				
♃ — 6	2 12 20	11 7 21+	57 30	☉'s upp. limb	
	15 56	3 44½			
	19 29	0 13+			
	23 10+	10 56 31½		☉'s low. limb	
	26 51	10 52 49::	55 26	☉'s upp. limb	6 39 56,8
	30 29½	49 13			
	34 3½	45 39+			
	37 46	41 55½		☉'s low. limb	
♀ — 7	16 11 24		25 6	α Lyræ	
	15 24				
	16 21 15—		23 22	Ditto	
	25 29				
	29 45				

Equal Altitudes of the Sun and Stars.

1769 July	Time per Clock of the equal Altitudes of the ⊙'s limb, and of ✱'s.		Zen. diſtance or points on the limb of the quadt. the nonius was ſet at.	⊙'s limb, and Stars obſerved.	Time per Clock when the ⊙'s cent. and ✱'s paſſed the merid. from the mean of the Obſervations.
	h ′ ″	h ′ ″	° ′		
⊙—16	Wound up the Clock				
☽—17	4 44 15	Clouds			
	48 34	9 52 17—			
	52 54	47 59	43 45	⊙'s upp. limb	
	57 27½	43 24½		⊙'s low. limb	
	5 1 56	9 38 55			7 20 33,5
	6 32—	34 14 :	41 12	⊙'s upp. limb	high wind
	Clouds	Clouds			
	5 16 8	9 24 42		⊙'s low. limb	
♀—21	16 2 4				
	6 6		25 6	α Lyræ	
	10 6½				
	16 15 56½				
	20 11		23 22	Ditto	
	24 26½				
♄—22	3 41 45½	11 35 12+			
	45 27	31 32—	55 6	⊙'s upp. limb	
	49 4	27 53			
	49 15	27 42		⊙'s low. limb middle wire	
	52 54½	11 24 4		Ditto 3^{d} or laſt wire	
	3 56 48½	11 20 9½			7 38 40,2
	4 0 35	16 25	53 1	⊙'s upp. limb	
	Clouds	12 42 : :			
	4 16 47½	11 0 11½	50 50	⊙'s upp. limb	
	4 20 47	10 56 23½		⊙'s low. limb	
	24 37+	10 52 23		Ditto	
	16 1 42½				
	5 43½		25 6	α Lyræ	
	9 44—				

Equal Altitudes of the Sun and Stars.

1769 July	Time per Clock of the equal Altitudes of the ☉'s limb, and of ✳'s.		Zen. diſtance or points on the limb of the quadt. the nonius was ſet at.	☉'s limb, and Stars obſerved.	Time per Clock when the ☉'s cent. and ✳'s paſſed the merid. from the mean of the Obſervations.
	h ′ ″	h ′ ″	° ′		
♄—22	16 15 33½				
	19 49—		23 22	α Lyræ	
	24 4				
♂—25	16 0 33				
	4 35½		25 6	α Lyræ	
	8 35½				
	14 25				
	18 40		23 22	Ditto	
	22 25				
♄—29	15 59 4				
	16 3 3½::		25 6	α Lyræ	
	7 5				
	Then cloudy				
Auguſt ☉— 6	4 2 35+	12 59 58½			
	6 13½	56 21	62 30	☉'s upp. limb	
	9 46+	52 46+			
	9 57	52 36		☉'s low. limb	
	13 33	12 49 1		Ditto	8 31 34,
	4 18 36	12 43 58+			
	22 17	Clouds	60 15	☉'s upp. limb	Very high winds
	25 54	12 36 42			
	29 42+	12 32 53		☉'s low. limb	
☽— 7	Clouds				
	Clouds		23 22	α Lyræ	
	16 17 59				
☿— 9			23 22	α Lyræ	
	16 17 15+				

Equal Altitudes of the Sun and Stars.

1769 April	Time per Clock of the equal Altitudes of the ⊙'s limb, and of ✱'s.		Zen. diſtance or points on the limb of the quadt. the nonius was ſet at.	⊙'s Limb, and Stars obſerved.	Time per Clock when the ⊙'s cent. and ✱'s paſſed the merid. from the mean of the Obſervations.
	h ′ ″	h ′ ″	° ′		
☿ — 9	17 25 24½				
	29 00—		25 45	α Cygni	
	32 32				
	37 43½				
	41 21		24 00	Ditto	
	44 56				
☿ — 16	17 22 49				
	26 25—		25 45	α Cygni	
	29 57				
	17 35 7½				
	38 44½		24 00	Ditto	
	42 20+				
♄ — 17	17 51 17—	21 57 23			
	54 58½	53 41½	21 41	α Cygni	
	58 38	50 2½			
	18 4 23				
	8 9½		19 53	Ditto	19 54 20,0
	11 55				
☽ — 21	17 49 47—				
	53 28		21 41	α Cygni	
	57 8—	Cloudy at the time correſponding.			
	18 2 52				
	6 39		19 53	Ditto	
	10 24				
♃ — 24	17 19 47½				
	23 22½		25 45	α Cygni	
	26 25—	Cloudy at the time correſponding.			
	Clouds				
	18 5 30		19 53	Ditto	
	9 17½				

Equal Altitudes of the Sun and Stars.

1769 Auguſt	Time per Clock of the equal Altitudes of the ☉'s limb, and of ✱'s.		Zen. diſtance or points on the limb of the quadt. the nonius was ſet at.	☉'s limb, and Stars obſerved.	Time per Clock when the ☉'s cent. and ✱'s paſſed the merid. from the mean of the Obſervations.
	h ′ ″	h ′ ″	° ′		
☉—27	Clouds 17 22 15½ Clouds		25 45	α Cygni	
	Clouds 17 34 36 38 11—		24 00	Ditto	
	23 30	Saw a Comet near			
Sept. ♀— 1	Saw the aforementioned Comet through a thick haze; it was moved to the eaſtward.				
♃— 7	Clouds Clouds 17 21 40		25 45	α Cygni	
	17 26 52 Clouds 34 2½	Rain in the night	24 00	Ditto	
☉—10	Cloudy with rain till about half paſt 4 in the morning, when the clouds broke in the eaſt, and I ſaw the Comet a little to the ſouth of Procyon; its tail extended nearly to the belt of Orion, and made a ſplendid appearance. Cloudy and rain in 8′ after.				
☽—11	Wound up the clock				
♀—15	17 23 53+ 27 32— 31 6	22 3 3+ 21 59 26— 21 55 50½	24 00	α Lyræ	
	17 40 25:: 44 8 Clouds	— — — 21 42 50 — — —	21 41	Ditto	19 43 28,5
	19 54 59½ 58 46— 2 32—	21 31 58+ 21 28 10+	19 41	Ditto	

Equal Altitudes of the Sun and Stars.

1769 Sept.	Time per Clock of the equal Altitudes of the ☉'s limb, and of ✳'s. h ′ ″	h ′ ″	Zen. diſtance or points on the limb of the quadt. the nonius was ſet at. ° ′	☉'s limb, and Stars obſerved.	Time per Clock when the ☉'s cent. and ✳'s paſſed the merid. from the mean of the Obſervations.
☽—18	17 22 46 26 24+ 29 59		24 00	α Cygni	
☿—20	17 53 5½ 56 53½ 18 0 39		19 41	α Cygni	
	Clouds 18 9 37½ Clouds		18 00	Ditto	
♃—21	8 10 33 15 20½ 20 9:	13 54 51 50 4½ 45 16	63 48	☉'s upp. limb	
	20 26 25 21	44 59— 40 6½		☉'s low. limb Ditto	11 3 5,6
	8 32 44: 38 8 43 27	13 32 36 Clouds Clouds	61 30	☉'s upp. limb	
	43 46 49 15	Clouds		☉'s low. limb	
	17 52 43 56 33½ 18 0 17½		19 41	α Cygni the ✳ fluttered	
	18 5 20½ 9 15½ 13 10		18 00	Ditto.	
♀—29	9 9 9 14 54 20 45	13 47 59 Clouds Clouds	63 24 Very thick and hazy	☉'s upp. limb	
	Clouds 18 6 15 10 10	Cloudy at the time correſponding to theſe	18 00	α Cygni	

Equal Altitudes of the Sun and Stars.

1769 Sept.	Time per Clock of the equal Altitudes of the ⊙'s limb, and of ✱'s.		Zen. diſtance or points on the limb of the quadt. the nonius was ſet at.	⊙'s limb, and Stars obſerved.	Time per Clock when the ⊙'s cent. and ✱'s paſſed the merid. from the mean of the Obſervations.
	h ′ ″	h ′ ″	° ′		
♀—29	— — —	Cloudy at the time corresponding to theſe			
	8 15 24		16 50	Ditto	
	19 28				
	18 26 14½				
	30 39		15 00		
	35 3½				
	18 53 8	— — —			
	57 52	20 18 39	12 14		19 38 15,5
	19 4 1	Cloudy			
♄—30	8 40 12	14 23 27½			
	45 10	18 34	66 55	⊙'s upp. limb	
	50 5	13 37			
	50 24	14 13 17½		⊙'s low. limb	
	55 27	8 16		Ditto	
	9 3 34	14 0 8			
	9 3	13 54 40	64 35	⊙'s upp. limb	11 32 14,4
	14 32	49 4			
	9 9 18	13 54 25			
	14 55	13 48 43		Ditto low. limb	
	20 41	43 2:			
	18 19 7	20 56 39	16 50	α Cygni	
	18 25 52½	20 49 53+			
	30 14	45 30	15 00	Ditto	
	34 41	41 5			19 37 52 7
☿ Oct 4	9 18 33	14 11 15½			
	24 12	— — —	65 50	⊙'s upp. limb	
	29 53	13 59 58			
	24 29	— — —			
	30 16	59 32		⊙'s low. limb.	11 45 17,6
	36 7	53 37::			

Equal Altitudes of the Sun and Stars.

1769 October	Time per Clock of the equal Altitudes of the ☉'s Limbs, and of ✳'s.		Zen. diſtance or points on the limb of the quadt. the nonius was ſet at.	☉'s limb, and Stars obſerved.	Time per Clock when the ☉'s cent. and ✳'s paſſed the merid. from the mean of the Obſervations.
	h ″	h ′ ″	° ′		
☿ — 4	9 44 35	13 45 15	63 36	☉'s upp. limb	
♃ — 5	18 24 1—	20 48 2	15 00	α Cygni	
	28 24	43 39			
	32 49	39 12			19 36 1,3
	18 49 52	20 22 10½	12 14	Ditto	
	55 39	20 16 25			
♂ —10	9 44 48	14 24 28:	67 23	☉'s upp. limb	
	50 48	14 13 30			
	56 50	Clouds			12 5 1,6
	9 51 5			☉'s lower limb	
	57 16				
	10 3 33	14 5 42			
♃ —12	9 20 33	15 2 3	71 00	☉'s upp. limb.	
	25 40—	14 56 53			
	30 49	51 47			
	25 55	14 56 36		Ditto low. limb	12 11 41,0
	31 10	51 25			
	36 26	46 8			
☽ —23	9 23 7—	Cloudy at the time corresponding	68 16	☉'s upp. limb	
	27 46				
	32 25				
	9 28 11				
	32 45				
	37 26½	15 59 39½		Ditto low. limb	
	9 45 16	15 51 45	75 54	☉'s upp. limb	
	50 18	46 40			12 48 52,1
	55 20	41 37½			
	9 50 35	46 24		Ditto low. limb	
	55 42	41 16			
	10 0 50	36 9			

Apparent Zenith Diſtances of the ⊙, ☽, and *'s.

1769 April	⊙, ☽, or *'s obſerved.	Apparent zen. diſtance on the meridian. °	′	″	Barom.	Ther.
☽ —— 10	Spica	64	45	24		
20	⊙'s upper limb	42	51	00	29 69	52
	Ditto lower limb	43	22	40		
22	Spica	64	45	32		
	Arcturus	34	26	20		
	♃'s center	71	21	36		
	☽'s lower limb	77	30	36:		
	Antares	80	38	40	29 52	42
25	Spica	64	45	16:	20 93	46
26	Spica	64	46	6		
	Arcturus	34	27	00	29 85	52
28	Arcturus	34	27	8:	29 92	49
29	Ditto	34	27	00	30 10	52
30	⊙'s upper limb	39	37	40	29 97	60
	Ditto lower limb	40	9	16:		
	Spica	64	46	00		
May 1	⊙'s upper limb	39	19	24	31 00	62
	Ditto lower limb	39	51	16		
2	⊙'s lower limb	39	33	26	30 20	59
3	Spica	64	46	00		
	Arcturus	34	26	52	30 05	55
	Antares	80	39	12		
4	⊙'s upper limb	38	26	16	30 00	56
6	Spica	64	45	50	30 12	48
	Arcturus	34	26	52		
7	⊙'s upper limb	37	36	00	29 94	59
	Ditto lower limb	38	7	56		
	Spica	64	45	50		
	Arcturus	34	26	48	29 87	47
	Jupiter's center	70	52	24		

Apparent Zenith Diſtances of the ☉, ☽, and ✳'s.

1769	☉, ☽, or ✳'s obſerved.	Apparent zen. diſtance on the meridian.	Barom.	Ther.	
		° ′ ″			
May 10	Spica	64 45 46			
	Arcturus	34 26 48	29 86	43	In obſerving the ☉'s zen. diſt. I ſet the ☉'s limb juſt (or ſcarcely) to touch the wire; which is the reaſon why the ☉'s diameter is in this obſervation ſo much greater then made before.
12	☉'s upper limb	36 17 16	29 60	54	
	Ditto lower limb	36 49 48			
	Spica	64 46 00:			
♄ —— 13	☉'s upper limb	36 2 42	29 52	58	
	Ditto lower limb	36 34 30			
14	☉'s upper limb	35 48 28	29 55	57	
	Ditto lower limb	36 20 2			Obſerved without the dark glaſs; it being a very thick fog.
	☽'s upper limb	55 50 56	29 63	49	
	Spica	64 45 55			
	Arcturus	34 26 52	29 69	44	
15	☽'s upper limb	60 54 40	29 69	44	
	Spica	64 46 00:			
18	☉'s upper limb	34 53 40	29 54	51	
	Ditto lower limb	35 25 12			
	Spica	64 46 00			
	☽'s upper limb	73 30 32			
	♃'s cent.	70 30 20	29 64	37	
23	☉'s upper limb	33 53 00	29 22	59	
	Ditto lower limb	34 24 32			
	Arcturus	34 26 52	29 34	55	
24	☉'s upper limb	33 41 48	29 41	64	
	Ditto lower limb	34 13 38			
June 11	☉'s upper limb	31 25 40	29 41	53½	
12	Ditto	31 22 00:	29 66	55	
	Arcturus	34 26 44			
14	☽'s upper limb	72 27 14	29 36	55	
15	☉'s upper limb	31 13 00	29 46	58	
17	Antares	80 39 12			
	☽'s upper limb	78 44 48	29 35	47	
18	☉'s upper limb	31 7 54	29 41	54	
	Ditto lower limb	31 39 42			

Apparent Zenith Diſtances of the ⊙, ☽, and ✱'s.

1769	⊙, ☽, or ✱ obſerved.	Apparent zen. diſtance on the meridian.			Barom.	Ther.
		h	′	″		
June 23	⊙'s upper limb	31	7	20	29 17	60
	Ditto lower limb	31	39	12		
26	⊙'s upper limb	31	12	6	29 38	5
	Ditto lower limb	31	43	56		
30	⊙'s upper limb	31	24	6	29 71	60
July 1	⊙'s upper limb	31	28	00	29 62	62
5	⊙'s upper limb	31	48	00	29 72	67
	Ditto lower limb	32	19	56		
6	Antares	80	39	20	29 75	57
9	⊙'s upper limb	32	14	46	29 57	63
13	☽'s upper limb	76	58	16	29 34	57
14	⊙'s upper limb	32	56	12	29 49	59
	☽'s upper limb	78	27	10	29 66	56
17	⊙'s upper limb	33	25	28	29 12	59
	Ditto lower limb	33	57	26		
22	Ditto	34	53	24	29 49	63
Auguſt 2	⊙'s upper limb	36	54	30	29 50	62
17	α Aquilæ	46	33	12	29 69	54
	☽'s upper limb	61	39	20	29 70	51
19	⊙'s upper limb	41	56	20	29 40	57
	Ditto lower limb	42	27	56		

To find the Error of the Line of Collimation of the Quadrant.

1769 April ♄—22 I ſet up a board at the diſtance of about 300 yards, painted black, with two white marks on it; the diameter of each white mark = 3½ inches, and the diſtance of their centers 11½ inches = the difference of the height of the center of the teleſcope, when the quadrant is inverted, and made the following obſervations.

	Zen. diſt. of the upp.mark.			
	°	′	″	
	89	50	00	The quadrant in its proper poſition.
		50	6	
			4	
			6	
			0	
			4	
			0	
Mean —	89	50	3	
	Zen. diſt. of the low.mark.			
	°	′	″	
	90	7	20	The quadrant inverted.
		7	20	
			18	
			14	
			16	
			20	
			20	
Mean —	90	7	18,3	
	89	50	3	
	179	57	21,3	
	180	0	0	
		2	38,7	
½=		1	19,3	To be added to the obſerved zenith diſtance.

☿—26 I took the teleſcope off the quadrant, and adjuſted the line of collimation ſomething nearer.

28 I found the error of the line of collimation of the quadt. in the ſame manner, as on the 22d, thus:

	Zen. diſt. of the upper mark.			
	°	′	″	
	89	51	00	Quadt. in its proper poſition.
		51	00	
		51	00	
		51	00	
		50	56	
		51	4	
		51	6	
		51	6	
		51	0	
		51	6	
Mean	89	51	2–	

	Zen. diſt. of the low.mark.			
	°	′	″	
	90	7	20	Quadant inverted.
		7	12	
			24	
			20	
			22	
			20	
			18	
			16	
			18	
			20	
			12	
Mean =	90	7	18,4	
	89	51	2--	
	179	58	20,1	
	180	00	00	
		2	39,9	
½ =		0	50	To be added to the obſerved zen. diſtance.

N.B. This method is given us by the Rev. Mr. Nevil Maſkelyne, Aſtronomer Royal, in his deſcription of Mr. Bird's aſtronomical quadt. publiſhed with his inſtructions for the obſervation of the late Tranſit of Venus, at the end of the Nautical Almanac of 1769, ſee p. 23, and may be followed with great accuracy; and to avoid the error that may fall on any two diviſions of the quadt. as many different diviſions may be taken as the obſerver pleaſes, by the ſhifting the board that has the marks on it, higher or lower.

For the Latitude of the Obſervatory at Cavan.

	☉ or ∗'s obſerved	App. zenith diſtances on the merid.	Refra.	☉'s px. in alt. hor. = 8″,5	∗'s ab. in decl.	∗'s nut. in decl.	Error of the line of colli. of the quad.	True zen. diſtances.	☉ or ∗'s true decl. at the time of obſervation	Latitude from each obſervation.
69 ril		° ′ ″	+ ′ ″	−	″	″	+ ′ ″	° ′ ″	° ′ ″	° ′ ″
10	Spica	64 45 24	2 0,5		−7,6	−6,3		64 48 29,6	9 57 00,0	54 51 30
20	☉'s center	43 6 50	0 53,1	5,8				43 8 56,3	11 42 55,2	51 51
22	Spica	64 45 32	2 2,4		−7,5	−6,2	1 19+	64 48 39,7	9 57 00,5	51 39
22	Arcturus	34 26 20	0 39,7		−5,9	−5,8		34 28 7,0	20 23 33,8	51 41
	Antares	80 38 40	5 41,0		−2,0	−2,7		80 45 35,3	25 53 56,7	51 39
25	Spica	64 45 16::	2 3,0		−7,5	−6,2		64 48 24,3::	9 57 0,6	51 24::
26	Spica	64 46 6	2 0,8		−7,5	−6,2	+ ′ ″	64 48 43,1	9 57 0,7	51 42
	Arcturus	34 27 0	0 39,1		−5,3	−5,8		34 28 18,0	20 23 33,6	51 52
28	Ditto	34 27 8	0 39,4		−4,8	−5,8	0 50	34 28 26,8	20 23 33,5	52 00:
29	Ditto	34 27 0	0 39,4		−4,8	−5,8		34 28 18,8	20 23 33,5	51 52
30	☉'s center	39 53 28:	0 47,2	5,4				39 54 59,8:	14 56 48,5	51 48:
	Spica	64 46 00	2 0,5		−7,4	−6,2		64 48 46,9	9 57 00,8	51 46
1	☉'s center	39 35 20	0 47,9	5,4				39 36 52,5	15 14 57,2	51 50
2	☉'s L. L.	39 33 26	0 47,7	5,4				39 34 58,3	15 32 50,9	51 55
3	Spica	64 46 00	2 0,7		−7,2	−6,2		64 48 37,3	9 57 1,0	51 36
	Arcturus	34 26 52	0 39,0		−3,8	−5,7		34 28 11,5	20 23 33,4	51 45
	Antares	80 39 12	5 36,0		−2,6	−2,6		80 45 32,8	25 53 57,0	51 36
6	Spica	64 45 50	2 3,1		−7,0	−6,2		64 48 29,9	9 57 1,2	51 29
	Arcturus	34 26 52	0 39,9		−3,2	−5,7		34 28 13,0	20 23 33,2	51 46
7	☉'s center	37 51 58	0 43,5	5,2				37 53 26,3	16 58 21,2	51 48
	Spica	64 45 50	2 2,4		−7,0	−6,1		64 48 29,3	9 57 1,3	51 28
	Arcturus	34 26 48	0 39,7		−3,0	−5,7		34 28 9,0	20 23 33,1	51 42
10	Spica	64 45 46	2 3,5		−6,9	−6,1		64 48 26,5	9 57 1,5	51 25
	Arcturus	34 26 48	0 40,0		−2,3	−5,7		34 28 10,0	20 23 32,9	51 43
12	☉'s center	36 33 32	0 41,6	5,1				36 34 58,5	18 16 48,4	51 47
13	☉'s center	36 18 36	0 48,0	5,0				36 20 1,8	18 31 36,0	51 38
14	☉'s center	36 4 15	0 40,2	5,0				35 5 40,2	18 46 4,5	51 45
	Spica	64 45 55	2 2,5		−6,5	−6,1		64 48 34,9	9 57 1,7	51 33
	Arcturus	34 26 52	0 39,6		−1,5	−5,7		34 28 14,4	20 23 32,7	51 47
18	☉'s center	35 9 26	0 39,8	4,9				35 10 50,9	19 40 46,0	51 37
	Spica	64 46 00	2 4,4		−6,1	−6,1		64 48 42,2	9 57 2,0	51 40
23	☉'s center	34 8 46	0 36,9	4,8				34 10 8,1	20 41 36,8	51 45
	Arcturus	34 26 52	0 38,2		+0,2	−5,6		34 28 14,8	20 23 32,2	51 47
24	☉'s center	33 57 43	0 36,5	4,7				33 59 4,8	20 52 44,3	51 49
11	☉'s U. L	31 25 40	0 34,0	4,4				31 26 59,6	23 8 38,7	51 26
15	☉'s U. L	31 13 00	0 33,4	4,4				31 14 19,0	23 21 23,6	51 30

7.

1769.

For the Latitude of the Obſervatory at Cavan.

	⊙ or ∗'s obſerved	Ap. zenith diſtanceson the merid.	Refra.	⊙'s px. in alt. hor. = 8″,5	∗'s ab in decl.	∗'snut. in decl.	Error of the line of colli. of the quad.	True zen. diſtances.	⊙ or ∗'s true decl. at the time of obſervation.	Latitud from ea obſervati
1769		° ′ ″	+ ′ ″	—	″	″	+ ′ ″	° ′ ″	° ′ ″	° ′ ″
June 17	Antares	80 39 12	5 34,8		—3.9	—2,4	0 50	80 45 30,5	25 53 58,1	51 3
18	⊙'s center	31 23 48	0 33,9	4,4				31 25 7,5	23 26 37,1	51 4
23	⊙'s center	31 23 16	0 33,2	4,4				31 24 34,8	23 27 5,6	51 4
26	⊙'s center	31 28 1	0 33,6	4,4				31 29 20,2	23 22 25,0	51 4
30	⊙'s U. L.	31 24 6	0 33,8	4,4				31 25 25,4	23 10 26,0	51 3
July 1	⊙'s U. L.	31 28 0	0 33,5	4,4				31 29 19,1	23 6 24,6	51 3
5	⊙'s center	32 3 58	0 33,7	4,5				32 5 17,2	22 46 20,6	51 3
6	Antares	80 39 20	5 31,1		—3,8	—2,3		80 45 35,0	25 53 58,6	51 3
9	⊙'s U L.	32 14 46	0 34,3	4,5				32 16 5,8	22 19 50,6	51 4
14	⊙'s U. L.	32 56 12	0 36,0	4,6				32 57 33,4	21 38 17,3	51 3
17	⊙'s center	33 41 27	0 36,2	4,7				33 42 48,5	21 8 52,0	51 41
22	⊙'s L. L.	34 53 24	0 38,4	4,8				34 54 47,6	20 12 42,8	51 42
Auguſt 2	⊙'s U. L.	36 54 30	0 41,4	5,1				36 55 56,3	17 40 4,4	51 50
17	α Aquilæ	46 33 12	0 59,5		+7,8	+3,9		46 35 13,2	8 16 25,9	54 51 39
									Mean of the whole	51 40

N. B. By comparing Mr. Flamſtead's obſervations with thoſe made by the Rev. Mr. Maſkelyne, Aſtronom Royal, in the years 1765 and 1766, Arcturus moves annually 2,″0144 Southward in declination: then fore 18″,6 is ſubtracted in the above from the declination of Arcturus, as ſettled from Dr. Bradley obſervations for the beginning of the year 1760.

T

The following are the difference of R. A. between the ☽'s limb and *'s, obſerved by wires placed in the focus of the eye glaſs of a reflecting teleſcope, that magnified 80 times. The teleſcope was ſupported by a polar axis placed in the meridian, and on a ſtrong ſtand, loaded with weight, which made it keep its poſition very ſteady..

1769 July	Time per Clock.						Difference of R. A. between the ☽'s limbs and *'s.
♃ 20	♓ ♑ paſſed the vertical wires in the reflector.			☽'s 2d or Eaſtern limb, paſſed the ſame vert. wires.			
	h	′	″	h	′	″	
	19	39	33	-	-	-	
		39	43	19	43	32½	
		47	13		51	13	
		47	24½		51	25	
		47	36		51	38	
	20	5	11	20	9	35½	
		5	21½		9	48	
		5	34		10	00	
		18	13½		22	57½	
		18	25		23	9	
		18	36			20+	
		18	48			32	
	20	19	00::			44½	
	20	25	17+		30	12	
			28			33½	
			39+			35	
			51			46	
		26	3		30	59	
		33	54		39	2	
		34	5		39	13	
		34	40		39	48	
		41	37		46	56	
		41	48½		47	7½	
	Clouds				47	19	
	Cl.					30½	After theſe, it got ſo hazy that I could not ſee the ſtar.
	20	42	23½		47	42½	

N. B. The * paſſed along the wire parallel to the equator, or moved in a line parallel to the ſaid wire. At the laſt obſervations the * was about 18 or 20′ north of the ☽'s center.

Difference

Difference of Right Aſcenſion between the ☽'s Limb and *'s.

July ♀—21	Time per Clock.					
	☽'s 2d limb paſſed the wires vertical to the equator.			♓ ♒ paſſed the ſame vertical wires.		
	h	′	″	h	′	″
	18	30	3½	18	33	53—
		30	15+		34	3
		39	41+		42	13½
		39	32	-	-	-
		40	4		42	36—
		40	15+	-	-	-
		40	27—		42	59
	18	53	39½	18	55	47
	19	5	7	19	6	58
			18		7	10—
		-	29½		7	20+
			41½		7	32
		5	53½		7	43+
		10	47		12	27½
		10	59		12	38½
		11	10+		12	49½
		11	21½		13	1
		11	34—		13	12½
	19	17	00	-	-	-
			12—		18	42—
			23		18	52½
			35+		19	4
		17	47		19	16
		22	42	-	-	-
		22	53½	19	24	14
		23	4½	-	-	-
		23	16		24	36½
		23	28+		24	48
		30	18	-	-	-
		30	30½		31	40
		30	42—		31	50
		30	53½		32	1
		31	5½		32	14

July ♀—21	Time per Clock.					
	♓ ♒ paſſed the wires vertical to the equator.			☽'s 2d limb paſſed the ſame wires.		
	h	′	″	h	′	″
	21	51	4	21	53	27½
		51	15½		53	39+
		51	26		53	50
		51	37½		54	2
		51	49½		54	14
	21	58	39	22	1	14+
		58	50	-	-	-
		59	1		1	36
		59	12+		1	48
		59	24		2	00
	22	5	50½		8	36
		6	1½		8	46
		6	12	-	-	-
		6	24		9	10
		6	36—		9	21
		13	9		16	18
		13	20		16	29½
		13	31		16	41—
		13	42		16	53
		18	14		21	19+
		18	25		21	30½
		18	36—		21	41½
		18	47		21	53
	22	18	59		22	5+
		28	12½		31	32—
		28	23+		31	43+
		28	34½		31	54½
		28	46		32	5
		28	57+		32	18
		37	11		40	43—
		37	22+		40	54½
		37	33+		41	4½
		37	44½		41	17
	22	37	56½		41	29

Immediately after theſe, it began to get foggy.

N. B. In all theſe obſervations the * moved parallel to the equator. At the beginning of the obſervations the * was North of the ☽'s center about 18′.

At 19h.47′ when I left off to take the occultation, the * was but little North of the ☽'s center; by eſtimation 3 or 4′.

The differences of A.R. after the occultation are very accurate, the ☽ being near the meridian, leaving the * nearly in a right line from her center.

Difference

Difference of Right Aſcenſion between the ☽'s Limb and ∗'s.

July ♀ —21	Time per clock.	
	☽'s 2d limb paſſed the wires vertical to the equator.	x ♒ paſſed the ſame vertical wires.
	19 33 33½	19 34 38
	33 45+	34 49
	33 56+	35 0½
	34 8	35 12—
	34 20+	35 24—
	41 5+	- - -
	41 16½	42 9
	41 28	42 20½
	41 40—	42 32—
	41 52	42 43+
	46 11	46 56½
	19 46 23+	19 47 8

Left off to take the occultation.

1769 Auguſt ☿ —16	Time per clock.	
	☽'s 1ſt limb paſſed the wires vertical to the equator.	A ſtar of the 5th mag. paſſed the ſame vert. wires.
	h ′ ″	h ′ ″
	18 4 30—	- - -
	4 43—	- - -
	4 55	18 25 1+
	5 6	25 12—
	5 18—	25 23

Cloudy immediately after.

A bright ſpot in the ☽ moved along the directing wire, or wire parallel to the equator, and the ∗ followed about 6′ North of ☽'s center.

Difference of Right Aſcenſion between the ☽'s Limb and *'s.

	Time per clock.		
1769 Sept. ♀—15	☽'s 2d limb paſſed the wires vertical to the equator.	16 Piſces paſſed the ſame vertical wires.	
	19 45 7—		Cloudy
	19—		
	31		
	42½		
	45 54		
	19 54 17½	19 56 50	The * about 18′ South of the ☽'s center.
	30	56 2—	
	42½	13	
	53	23+	
	55 5	57 34+	
		then cloudy	
	20 29 16½	20 30 44	
	28	30 56	
	40+	31 8—	
	51½	31 19	
	30 3½	31 30	
	34 33+	36 53+	The * by eſtimation 24′ South of the ☽'s center.
	45+	36 5	
	58—	16+	
	35 9	27	
	35 20½	36 38+	
	41 13+	- - -	
	25½	42 33	
	38—	44+	
	48½	55+	
	42 0½	43 7—	
	46 16+	- - -	In theſe the ☽'s center and * nearly at the diſtance of the field of the teleſcope, or as near the ends of the wires as the ☽'s limbs would admit of.
	28+	- - -	
	40	20 47 41	
	51½	47 52	
	47 3½	48 3½	
	50 57—	51 49	
	51 8	52 0	
	51 20½	52 12	
	56 39	57 23	
	56 50+	57 33½	
	57 1½	57 45+	

Note. In all theſe obſervations the * was often tried if it would keep the wire parallel to the equator, after moving it off the wire, and bringing it on again (by means of the vertical ſcrew). For in theſe obſervations I was obliged to bring the ſtar more Southward after I had made it keep the wire, before I brought it back for the ☽; otherwiſe the ☽'s center would not follow through the field of the teleſcope; and I always found that it returned again to keep the wire with great accuracy: the wires very ſeldom wanting any alteration.

3 Differences

Difference of Right Aſcenſion between the ☽'s Limb and *'s.

Time per Clock.

Sept. ☿ —15	☽'s 2d limb paſſed the wires vertical to the equator.			16 Piſces paſſed the ſame vertical wires.			
	h	′	″	h	′	″	
	21	4	14+	The * would not follow through the field of the teleſcope.			
			26—				
			38				
			49				
		5	1				
☿ —20	20	24	34	20	26	13	
		24	47+		26	25½	
		24	59½			37—	
		25	12—		26	49	
	20	31	54—	20	33	12½	
		32	6			24—	
		32	18½		33	36	
		37	27—		38	31—	
		37	38½			42½	
		57	51½		38	54+	
	10	42	51—	—	—	—	
		43	4		43	53	
			17		44	5½	
			29—			17	
		43	41—		44	5+	
	20	50	55—		51	23½	
		51	7½		51	35½	
		58	29		58	37	
		58	42		58	50	
		58	56+		59	3	
		59	8		59	14½	
	20	59	21	10	59	26	
	21	1	—	the * 2″ after the ☽'s limb.			These may not be ſo accurate as the above.
	21	2		The ☽'s limb and * equal in A. R. as near as could be judged.			
	21	3	35	The ☽ followed the * 15″ in time.			
	21	7	50	Clouds			
	21	11		Clear			

Time per Clock.

Sept. ☿ —20	16 Piſces paſſed the wires vertical to the equator.			☽'s 2d limb paſſed the ſame wires.		
	h	′	″	h	′	″
	22	23	41	22	27	14
			54			27+
		24	6+			40+
			17+			52
		24	29½		28	4+
		30	47		34	40
		31	1—			53
			13		35	6+
			25			18
		31	37		35	30+
		43	19½		47	42
			33+			54½
			44		48	6½
	22	43	56	22	48	19+
	22	51	32+	22	56	15—
			45½			28
			58			41—
		52	9+			52½
		52	21½		57	5+
	23	2	56—	23	8	7—
		3	9—			20
			22+			32
			33+			44½
		3	45+		8	57
		15	16—		20	55
			29		21	9—

The Star was to move along the equatorial wire in all theſe obſervations, and I look upon the whole to be very good; particularly thoſe after the occultation, as the difference of declination of the ☽'s center and Star was by eſtimation not more than 4 or 5′, nor even ſo much at the laſt obſervations.

Difference of Right Aſcenſion between the ☽'s Limb and *'s.

Sept.	Time per Clock.					
☿ —20	16 Piſces paſſed the wires vertical to the equator.			☽'s 2d limb paſſed the ſame wires.		
	h	′	″	h	′	″
	23	15	41+	23	21	12+
			41+			33
		16	+		21	46−
		48	4		54	58½
			16+	—	—	—
			29+		55	25
	—	—	—			37−
	23	48	53	23	55	48½
	0	3	16	0	10	44½
			28+		10	58
			41		11	11
		3	53−		11	23
		—	—	0	11	35
	n		cloudy			
	☽'s 2d limb paſſed the wires vert. to the equator.			h Leonis paſſed the ſame vertical wires.		
☽ —25	2	52	2	2	54	53
			15½	—	—	—
			28−		55	16½
			39−		55	28+
		52	50½		55	38
	2	58	3−	3	0	41−
			15½		0	52½
			28−		1	4−
			39+		1	15½
		58	51−	3	1	27−
	3	3	54		6	18½
		4	6½			30+
			19−			43
			30			52
		4	42½		7	5
	3	15	10−		17	12
			22+			24
			34½			36+
			45½			47
		15	57+		17	58½

At theſe obſervations the Star followed the ☽'s center along the wire parallel to the equator.

Difference of Right Aſcenſion between the ☽'s Limb and ✱'s.

	Time per Clock.						
1769 Sept.	☽'s 2d limb paſſed the wire vertical to the equator.			h Leonisa paſſed the ſame vertical wires.			
☽—25	h	′	″	h	′	″	
	3	21	9+	3	23	0	At theſe obſervations the Star followed the ☽'s center along the wire parallel to the equator.
			22			11½	
			34			24—	
			45½			34	
		21	57		23	46	
		27	38½		29	15½	
			51			27	
		28	3			39	
		28	15—			50	
	3	28	26		30	1½	
		34	49½	—	—	—	
		35	2		36	23½	
			14½		36	35	
			25+		36	46—	
		35	37		36	57	
	3	44	Clo.	—	—	—	At theſe obſervations the Star was about 4′ north of the ☽'s center.
	3	55	11	—	—	—	
			24—	3	56	6	
			34			18	
			47			28½	
		55	58		56	40	
		58	29½		59	6	
			42+			18	
			53+			29	
		59	5		59	41	
	Then cloudy						
	4	15		Clear			
	δ ♈ paſſed the wires vertical to the equator.			☽'s 2d limb paſſed the ſame wires.			
Oct.	h	′	″	h	′	″	
☽—16	4	9	18	4	12	10	
		9	31			22+	
		9	44½			36	
		9	55+			48	
	4	10	8	4	13	0½	

Difference of Right Aſcenſion between the ☽'s Limb and ∗'s.

1769 Oct.	Time per Clock						
	δ ♈ paſſed the wires vertical to the equator.			☽'s 2d limb paſ- the ſame wires.			
☽—16	h	′	″	h	′	″	
	4	16	40		19	51—	The ∗ 10′ ſouth of the ☽'s center, and the ∗ paſſed along the middle wire.
		16	52+			4	
		17	4			16½	
			16			28	
	4	17	28		20	40½	
		22	50		26	14	∗ moved along the lower wire.
			3			27	
			15			40—	
			26			51½	
		23	38		27	4—	
		29	33		23	11	The ∗ moved along the middle wire, after theſe I ſet the wires that the ∗ paſſed exactly along the lower wire.
			46—			24	
			58			37	
			9			59	
		30	21—		34	1	
	4	54	47			21	
			59½			33	
			11½			46+	
			23			57	
		55	35+	5	0	10+	
	5	4	52		9	47+	The ∗ 15′ ſouth of the ☽'s center.
		5	4	—	—	—	
		5	15½		10	11	
	5	5	27	5	10	24	
	5	12	25	5	17	37½	
			38			51—	
			50+			4—	
		13	2			15+	
		13	14		18	27½	
	5	25	46½		30	26½	
		25	59			39:	
		26	11½			51—:	
		26	23		31	2½:	
		26	35		31	16	

Difference of Right Aſcenſions between the ☽'s Limb and ☽'s.

1769 Oct.	Time per Clock.						
	δ ♈ paſſed the wires vertical to the equater.			☽'s 2d limb paſſed the ſame wires.			
	h	′	″	h	′	″	
☽—16	5	38	4+	5	44	13	
			17+			26+	
			29			40—	
			41—		44	51	
		38	53—		45	3:	
23				Then cloudy			
	☽'s 2d limb paſſed the wires vertical to the equator.			a ſtar of 6th mag. paſſed the ſame vertical wires			
☽—23	3	39	55½	3	43	16	The ✳ about 6′ north of the ☽'s center.—The wires faintly illuminated and the ✳ alſo appeared very faint.
			8—			28	
			20—			40	
			30½			50	
		40	42+		44	1	
		46	32+		49	40	The wires very indifferently illuminated, and the ✳ ſtill appeared faint.
			45—		49	52	
		46	57		50	6	
		47	8+	—	—	—	
		47	20½		50	27—	
		55	3+		57	55	
			16—		58	7+	
			28—			19	
			38½			29½	
		55	50½		58	41	
	4	1	21½	4	4	1	
	4	53	9½		54	14	
			20+			25	
		53	$32\frac{2}{1}$		54	36—	
	5	1	41+	—	—	—	Theſe Numbers are a little dubious.
		1	54		2	43	
		2	6—		2	55½	
		2	16½		3	6½	
		2	30—		3	18—	

Clouds now began to interrupt the obſervations. Clear a few minutes before the ☽'s limb, and ✳ paſſed the wires at the ſame time: and at

5 32 12 [The ☽'s 2d limb and ✳ was equal in right aſcenſion: the ✳ by eſtimation 20′ north of the ☽'s center: then cloudy.

The Tranſit of Venus.

1769 June ♄ — 3	Time per clock h ′	
	3 18	Rain
	3 45	Viewed the ☉'s diſk with the reflector (mag. 128 times), and ſaw nothing more than ſome large irregular black ſpots, with a black ſtreak very near the edge of the ☉'s limb on the Eaſtern ſide.

	In.	Ten.	Non.	Mag.	
Adjuſtment of the nonius on the ſide of the teleſcope, for diſtinct viſion for the contacts	0	35	15	128	times
Ditto for the micrometer	0	30	5	62	
Ditto for the wire eye glaſs	0	25	23		

	10 30	Cloudy
The air not quite clear	11 17 53	The external contact of Venus and the Sun's limb.
Ditto more denſe	35 30	The contact ſeemed to be formed by judging by their peripheries.
	36 8	Internal contact, the thread of light broke out.

In.	Ten.	Non.		
0	1	16	to the right-hand of 0	Venus's (horiz.) diameter. Thoſe made to the left hand are ſet down where the nonius coincided, the compliment of which to 25 muſt be uſed.
0	1	8	to the left hand of 0	
0	1	13	right	
0	1	6	left	

12 12	Cloudy			
	4	0	20	The ☉'s horizontal diameter, hazy.
	4	0	20	
		1	00::	
12 21	Cloudy			
	0	1	7	to the left-hand of 0 — Venus's diameter.
		1	14	to the right-hand of 0
		1	10	left-hand

After theſe, I immediately extended the glaſſes again for the Sun's diameter, but was prevented by clouds from doing any thing farther.

12 45	Cloudy
50	Ditto

Eclipſes

Eclipſes of Jupiter's Satellites, Occultations of the ☽ with the fixed ✱'s, and other Phænomena.

1769	Time per clock h	′	″	Apparent Time h	′	″	
April 5	1	33	12	13	49	36	Immerſion 1ſt ſatellite of Jupiter.
June 3	11	17	53	6	41	13	External contact of Venus's and the Sun's limb.
	11	36	8	6	59	25	Internal ditto, the thread of light broke out.
	22	49	28	18	11	1	The eclipſe of the Sun began.
July 1	15	43	34	9	20	52	Emerſion 1ſt ſatellite of Jupeter. Twilight very ſtrong, yet the ſatellites appeared well.
21	20	19	14	12	42	16	Immerſ. ✱♒ into the ☽'s enlightened limb.
	21	33	27	13	56	18	Emerſ. of ditto from the ☽'s dark limb.
Sept. 20	21	17	35	10	16	20	Immerſ. of 1ſt ✱♉ into ☽'s light limb.
	21	48	54	10	47	35	Emerſ. of ditto from ☽'s dark limb.
	21	57	7	10	55	46,6	Em. of 2d ✱ ♉ from ☽'s dark L. dubious to 3 or 4″.
Sept. 25	4	26	15	17	8	2	Immerſ. of h ♌ into the ☽'s light limb. Twilight pretty ſtrong, but not to render the obſervation in the leaſt dubious.
Oct. 16	3	50	46	15	23	33	Emerſ. of δ♈ from the ☽'s dark L. Immediately after, a very thin flying cloud paſſed over the ☽; but I believe no part of it obſtructed the obſervation.
Nov. 9	23 or 23	35 or 36	30 or 30	9 or 9	44 or 45	42 or 42	19 ♓ immerged into the ☽'s dark limb. The ſecond is true to a ſecond, but which of theſe minutes is true, was rendered dubious by accident.
Nov. 21	15	56	40	15	19	50	The ✱ ι ♌ ſeemingly emerged from the ☽'s dark limb, but rendered a little dubious by flying hazy clouds.

Obſervations of the ☉, ☽, and *'s paſſing the Meridian, made with a Tranſit Inſtrument, the length of the Teleſcope 4 feet, having 2 Object Glaſſes, and magnifying 50 times.

1769
Sept.
♃—21 The tranſit inſtument, ſent by the Royal Society, was brought to Cavan.
☽—25 Began to ſat up the ſaid inſtrument.
♀—29 Examined the line of collimation of the tranſit inſtrument, and found it very much out.
♄—30 Brought it very near, and found the level very good.
Oct.
☉— 1 Brought the line of collimation quite exact (by many trials), uſing a diſtinct object at the diſtance of about two miles.
☽— 2 At noon cloudy; in the evening it began to clear.

	°	′	″	h	′	″
January 1, 1750, the mean A R. of the Pole * =	10	42	37			
Annual preceſ. 158″ then the preceſ. to this time =	+	52	00			
Aberration in A.R.	+	8	34			
Nutation in A. R.	+	1	30			
Appt. A. R. the 2d of October 1769.	11	44	41	0	46	59
Clock too ſlow for ſidereal time by the obſervations of α Cygni made on the 30th of Sept.					— 56	27
Clock loſes in the interval of time between α Cygni's paſſing the meridian and pole *				—	0	4
Pole * tranſits the meridian, October 2d, at				23	50	28 by the clock.

At this inſtant of time ſhewn per clock, I brought the middle wire to biſect the Pole *, and after took the paſſage of the following *'s over the meridian.

1ſt wire	2d wire	Time per clock of paſſing the meridian.	4th wire	5th wire	
′ ″	′ ″	h ′ ″			
		3 26 10+			Aldebaran
		4 6 54½			Rigel
		4 46 7			α Orion
		5 38 24—			Syrius

Note, Juſt before I brought the wire to the pole *, I ſet the axis of the tranſit inſtrument horizontal by the level; and at $3^h\ 52'$, I examined it again by the level, and found it very exact. In the morning I placed a mark in the meridian about ¾ of a mile North, and took particular notice what natural marks the middle wire cut on the ſummits of two hills, the one North about two miles, and the other South at a greater diſtance; theſe ſerved after as compleat marks in the meridian: and few meridians there are, I believe, of ſuch a length. At firſt, before I could depend upon the inſtrument's keeping its direction all night; I ſent a man to place a candle in the center of the mark placed in the meridian, and it was very ſeldom that I found it vary in the leaſt: what ſmall difference might ſometimes appear, was more probably owing to the placing of the candle, than the movement of the inſtrument.

Obſervations made with the Tranſit Inſtrument.

1769 Oct.	1ſt wire	2d wire	Time per clock of paſſing the meridian.	4th wire	5th wire		
	′ ″	′ ″	h ′ ″	′ ″	′ ″		
♂ — 3		25 0	3 25 46½	26 32	27 18	Aldebaran	
	14	58	4 45 43 .	46 28	12+	α Orion	
			5 38 00—	45+	39 31 : :	Syrius	
☿ — 4	Clouds	Clouds	11 43 13 : :			⊙'s 1ſt limb	
			11 46 23—		47 51+	⊙'s 2d limb	
	— —	— —	— — —	3 26 10	26 56+	Aldebaran	
	37+	22+	4 6 7+	52—	37	Rigel	
	52	36+	4 45 21½	6+	50½	α Orion	
	3½	50	5 37 37—	22½	39 8½	Syrius.	
♃ — 5	15+	0—	18 37 45½	38 30	15+	γ Aquilæ	
	Clouds	Clouds	Clouds	45 44	28½	α Orion	
	42½	29—	5 37 15½	1	38 47½	Syrius	
♀ — 6			23 49 12	— —	— —	Pole ✳, above the Pole	
		53	3 24 40—	25½	Clouds	Aldeb.	
	53	38—	4 5 23	8—	52+:	Rigel	
	7	51	4 44 36+	21	46 5+	α Orion	
	35 19	5	5 36 52—	38	24	Syrius	
			6 29 5½	29 50	34+	Procyon	
♂ —10		3 12	12 3 57	— —	— —	⊙'s 1ſt limb	Tried the line of collimation and the horizontal poſition of the axis, and found both correct.
			6 7	6 51+	36½	⊙'s 2d limb	
	22½	7+	18 35 53	— —	22½	γ Aquilæ	
	38 37	22—	18 40 7—	51	41 36	α Aquilæ	
	— —	22 35	20 23 22—	24 7½	24 54	☽'s 1ſt limb	
			20 27 51	28 37	29 23½	γ Capricorn	
			33 15	34 0—		1ſt C. ditto	
	Clouds		3 23 10½	57—	Clouds	Aldeb.	
☿ —11		2 46+	4 3 32—	4 16+	— —	Rigel	
		59½	4 42 45—	29	14—	α Orion	
	28—	14	5 35 1—	35 46½	33	Syrius	
	44½	29½	6 27 14½	58½	43	Procyon	
♃ —12		9 50½	12 10 35	— —	— —	⊙'s 1ſt limb	
			12 46—	13 30	14 15	⊙'s 2d limb	
	3 1½	3 47—	22 4 33	5 18+	3+	☽'s 1ſt limb	
			4 3 10—	54+	39½ :	Rigel	
	Clouds	Clouds	42 23—	7+	43 52—	α Orion	

Obſervations made with the Tranſit Inſtrument.

1769	1ſt wire	2ſt wire	Time per clock of paſſing the meridian.	4th wire	5th wir		
Oct.	′ ″	′ ″	h ′ ″	′ ″	′ ″		
♀—13		38 14+	18 38 59½	39 44½	29—	α Aquilæ	
	55 42	5 28—	22 57 14—	57 59	58 44+	☽'s 1ſt limb	
	17½	2½	4 2 48—	3 32	4 17	Rigel	
	31 +	16—	42 1—	45	30—	α Orion	
	45		5 34 17½	3½	50	Syrius	
	1½		6 26 31+	15½	— —	Procyon	
♄—14			12 19 27+	20 11½	20 56	☉'s 2d limb	Very windy.
	52½	51 39	23 52 26—	53 11+	53 58—	☽'s 1ſt limb	Then cloudy.
☉—15	19 6+	19 52—	12 20 37+	— —	— —	☉'s 1ſt limb	} windy.
			22 47	23 31½	24 16+	☉'s 2d limb	
	45—	29+	18 38 15—	38 59	39 44	α Aquilæ	
			41 47			ξ Ditto.	
☽—16	28—	12½	12 23 58			☉'s 1ſt limb	
			26 9—	53+	27 39	☉'s 2d limb	
	36 22	7—	18 37 52	38 36½	39 21	α Aquilæ	
	24+	9—	4 40 54	38½	23	α Orion	
♃—19	47	35 32	12 36 17½	37 2+	Cloudy	☉'s 2d limb	
	14½	59—	18 36 44	29—	13½	α Aquilæ	
			1 47 22½			α Ceti	
		7+	2 30 56	44+	32½	η Pleiadum	
		2	3 19 49—	20 34½	21 21—	Aldebaran	
		59 48	4 0 33	1 17½		Rigel	
		00½	4 39 46—	40 30	16—	α Orion	
	26	13+	4 58 2	58 50—	38	η Gemini	
	4 28½	16	5 6 5	6 52½		μ Ditto	
	8 48½	9 37+	5 10 27—	11 16+	5—	☽'s 2d limb	
♀—20	35 59	36 44+	12 37 29			☉'s 1ſt limb	
			39 41½	40 26½	41 11	☉'s 2d limb	
			4 0 11+	59—	1 41—	Rigel	
	0+	45—	4 9 30	10 14+	59—	γ Orion	
		20 18+	5 21 5	51		γ Gemino	
	11 1	11 49	6 12 38	13 26+	14 14+	☽'s 2d limb	
	22 24	8+	6 23 53+	24 38—	22	Procyon	
	6 29 10	Clouds				Pollux	

Obſervations made with the Tranſit Inſtrument.

1769	1ſt wire	2d wire	Time per clock of paſſing the meridian.	4th wire	5th wire		
Oct.	″	″	h ′ ″	′ ″	′ ″		
♄ —21	56 17+	57 2½	22 57 49 : :	Cl.	Cl.	γ Pegaſi	
	Cl.	Cl.	3 19 4	Cl.	Cl.	Aldeb.	
	Cl.	- - -	Cl.	39 45+	Cl.	α Orion	
☉—22	- - -	24 53	18 35 38	36 23—	7+	α Aquilæ	
	21 39	22 23+	6 23 8+	53—	24 37	Procyon	
	- - -	- - -	- - -	6 28 57—	28 47 :	Pollux	
	4 35+	5 22	8 6 8½	6 54½	7 41	☽'s 2d Limb	
			8 51 57+	52 42½	28	Regulus	
☽ —23	46 16	47 1	12 47 46½			☉'s 1ſt Limb	
			49 59—	50 44	51 29+	☉'s 2d Limb	
	45+	34 30—	18 35 15	59+	44+	α Aquilæ	
♂ —24	54½	39	6 22 24	8		Procyon	
		30½	6 26 21+	11½		Pollux	
	10 30½	18+	7 12 5	12 52	13 39	Saturn's center	
	49 40+	26	8 51 12—	57	42½	Regulus	
	46 23	47 8	9 47 54	48 39	Clouds	☽'s 2d Limb	
☿ —25	53 10+	55+	12 54 41½			☉'s 1ſt Limb	
			Clouds	Cl.	57 39+	☉'s 2d Limb	
	33 00	45—	18 34 30—	14+	59—	α Aquilæ	
			10 Examined the line of collimation, and axis with the level, and found both very good.				
♃—26	15 38½	25—	3 17 11½	17 57	43½	Aldeb.	From this time to the fourth of November cloudy, and rain.
Nov.							
♄— 4		29 12+	13 29 59			☉'s 1ſt Limb	
			32 14—	32 59½	45½	☉'s 2d Limb	Hazy, the limb appeared faint.
	10 40	11 28½	18 12 17+	13 7	14 54	☽'s 1ſt Limb	
	16	30 0½	18 30 46—	30½	32 15	α Aquilæ	
	Great rains in the night.						

Obſervations

Obſervations made with the Tranſit Inſtrument.

1769 Nov.	1ſt wire	2d wire	Time per clock of paſſing the meridian.	4th wire	5th wire	
	′ ″	′ ″	h ′ ″	′ ″	′ ″	
☉ — 5		35 4	13 35 50½	36 36½	Cl.	☉'s 2d limb
		38½	18 30 24—	8½	53	α Aquilæ
		58 8	18 58 54½	59 40		2 β Capricorn
	0 42	1 29+	19 2 17+	3 4½	3 52	☽'s 1ſt limb
			19 13 20—	14 6—		N°. 439 zodiac, de la Caille
			19 18 25½			Teleſcopic ſtar a little N. of the fore-
			19 31 3	31 47½		μ Aquarii [going one.
	Cloudy all the morning part.					
☿ — 8	Tried the line of collimation, and the horizontal poſition of the axis, and found both exact.					
			11 46 48	47 32+	48 16½	Venus's center
♃ — 9	46 37—	23	13 48 10			☉'s 1ſt limb
			50 26	51 12+	58½	☉'s 2d limb
		28 10	18 28 55½	29 40	24½	α Aquilæ
	7 17—	8 1½	20 8 46+	31—	15+	β Aquarii
	20 44	21 29—	20 22 14	58½	23 43+	ε Pegaſi
			22 17 27			γ Piſcium
	18 29+	19 15	22 20 0½	20 46	21 31	☽'s 1ſt limb
		23 13	22 23 58—	24 42		19 Piſcium
♀ — 10	50 17	51 4	13 51 51			☉'s 1ſt limb
			54 7	53½	40—	☉'s 2d limb
	4—	48+	18 28 34—	18	3—	α Aquilæ
	40 46—	41 31½	21 42 17½	3—	43 48+	α Pegaſi
	11 28—	12 13½	23 13 0—	46—	14 31+	☽'s 1ſt limb
	52½	51 37½	3 52 23—	7—	53 52—	Rigel
	0 12+	0 56	4 1 41	2 26—	10	γ Orion
	6½	51—	4 31 36	32 20+	33 5	α Orion
♄ — 11	42—	26+	18 28 12—	56+	41—	α Aquilæ
☉ — 12	40 50½	41 39+	0 42 28—	15+	44 3—	α Arietis
	8 15+	9 3	1 9 51½	10 39—	11 26+	☽'s 1ſt limb
		13½	2 22 2	22 50		η Pleiadum
	9 22	10 8	3 10 55—	40	27—	Aldeb.

Obſervations made with the Tranſit Inſtrument

1769 Nov.	1ſt wire ′ ″	2d wire ′ ″	Time per clock of paſſing the meridian. h ′ ″	4th wire h ′ ″	5th wire h ′ ″	
♂—14		8 13—	14 9 00—	Clouds	10 33	☉'s 2d limb
	25 36	21—	18 27 6	51—	35	α Aquilæ
	Clouds	Clouds	Clouds	Clouds	0 43 20	α Arietis
	39—	25	3 10 12—	10 57+	11 43+	Aldebaran
			3 15 55—	42½		τ Tauri
	22 36	23 25½	3 24 15+	25 5—	25 54—	☽ 2d limb.
☿—15	8 53+	9 40	14 10 27+			☉'s 1ſt limb
			12 45—	13 31+	14 18	☉'s 2d limb.
♃—16			14 16 31	17 17	4	☉'s 2d limb
		40 12—	0 41 00	41 48—	42 36—	α Arieties
	54½	40½	3 9 27	13—	10 59—	Aldebaran
	54½	39—	4 29 24—	8+	53	α Orion
	9 32½	10 19	5 11 5+	11 51	37½	γ Gemino
	36 24—	37 13 ::*	5 38 1+	38 50	39 39—	☽'s 2d limb
	The poſition of the inſtrument in the morning was about a ſecond (in the equator) too much Eaſtward for Stars to the South of the Zenith.					
♀—17		39 50+	0 40 38+	26+	42 14	α Arietis
	7 33½	19	3 9 6—	51½	37½	Aldebaran
	12 2½	47	6 13 32—	14 16	Clouds	Procyon
	38 31+	39 19+	6 40 7+	40 54½	41 42½	☽'s 2d limb.
♂—21	37 33—	38 21—	0 39 9	57	44½	α Arietis
	6 4—	6 50—	3 7 36+	22	8	Albebaran
		48	4 27 33	17½	2½	α Orion
				10 4 0½	4 45	ε ♌
	10 1½	10 47	10 11 33—	12 17½	13 3	☽'s 2d limb.
☿—22	58 10	58 56	10 59 42	0 27+	1 13+	☽'s 2d limb
	Clouds	Cloudy	Clouds	12 50 17	12 51 4	Arcturus.
♃—23	21½	40 9—	14 40 57—	44		☉'s 1ſt limb
		28	43 16	44 3	50+	☉'s 2d limb
	22 17	1½	18 23 47	31+	16+	α Aquilæ.

* Occaſioned by a fluttering more than common.

Obſervations made with the Tranſit Inſtrument.

1769	1ſt wire	2d wire	Time per clock of paſſing the meridian.	4th wire	5th wire	
Nov.	′ ″	h ″	h ′ ″	′ ″	′ ″	
♀ —24			12 48 45½	49 32½	50 20—	Arcturus.
☽ —27 ♂ —28	Packing up the Inſtruments.		Tried the line of collimation, and found it good.			
Dec. 7	Sent off the Inſtruments for Dublin.					

Charles Maſon.

XL. *A Letter from M.* Pingré, *of the Royal Academy of Sciences at* Paris, *to the Rev. Mr.* Maſkelyne, *Aſtronomer Royal*, *F. R. S.*

SIR,

Read Dec. 6, 1770. I Should long ere now have ſent you my American obſervations, could I have preſumed you would have ſo far honoured them as to receive them with pleaſure. With your permiſſion, Sir, I ſhall in future direct to you, and at preſent ſend you my ſeveral obſervations in chronological order.

At Fort Royal in Martinico, on the new baſtion, emerſion of the firſt ſatellite, the 12th of May, 1769, at $16^h\ 28'\ 23''$ apparent time, ſerene ſky; the ſatellite very near ♃, with an achromatic teleſcope of five feet, having only two glaſſes for the object glaſs, and two plano-convex eye-glaſſes; the magnifying power about 140 times, made by M. l'Eſtang at Paris.

The 13th, emerſion of the third ſatellite, at $10^h\ 01'\ 00''$ apparent time, very ſerene, the ſame teleſcope; at $10^h\ 01'\ 22''$ by M. de Fleurieu, captain of the king's frigate Iſis, with an achromatic teleſcope 2 $\frac{1}{2}$ feet long, made at London by Mr. Dollond, a two-glaſs object-glaſs, and two plano-convex eye-glaſſes, the magnifying leſs than the former, but much clearer.

The 14th, emerſion of the firſt ſatellite at $10^h\ 57'\ 14''$ apparent time, M. de Fleurieu with the ſame teleſcope. I had been much fatigued, and quitted my poſt.

The ſame day, by ſeveral meridian altitudes of ſtars taken ſome to the North, ſome to the South, I found the latitude $14^\circ\ 35'\ 49\frac{1}{2}''$. M. de Fleurieu made it $14^\circ\ 35'\ 54\frac{1}{2}''$.

The 15th the rains began.

At Cape Francis in the iſland of St. Domingo.

June 3 we firſt perceived Venus entering on the Sun's diſk.

	h	′	″	
at	2	26	$14\frac{1}{2}$	apparent time, with Dollond's $2\frac{1}{2}$ feet teleſcope. M. de Fleurieu.
	2	26	$16\frac{1}{2}$	apparent time, with an achromatic of 3 feet, by l'Eſtang. M. la Filiere.
	2	26	$20\frac{1}{2}$	apparent time, with a common teleſcope of 2 feet, only 2 lenſes. M. des Saqui Tourés.
	2	26	$12\frac{1}{2}$	apparent time, with a five-feet achromatic. Myſelf.

After having given our eyes ſome reſpite, we returned to the teleſcopes; and M. de Fleurieu perceived a luminous little circle all round Venus, not yet entered more than about one third of her diameter. This luminous thread made, to all appearance, a perfect circle with the part of the circumference of Venus already advanced on the ſolar diſk. I likewiſe obſerved the ſame phænomenon, but a good while after M. de Fleurieu.

Venus appeared totally entered

	h	'	''	
at	2	44	45	M. de Fleurieu.
		44	41	M. le Chev. de la Filiere.
		44	50	M. Saqui des Tourés.
		44	44	Myſelf.

During both theſe obſervations, every thing was quiet and ſtill, not a word uttered, to intimate that any one had obſerved the contact.

Stormy weather almoſt every night hindered us from obſerving the eclipſes of the ſatellites. However, the 10th of June proving a clear night, afforded us an opportunity of determining the latitude of our obſervatory; which by meridian altitudes of ſeveral ſtars, both to the North and South, I determined to be 19° 47′ 03″. The new church of the Cape, ſituated nearly in the middle of the town, may be about 20″ or 25″ more Southward than our obſervatory, whence its latitude 19° 46′ 40″ North.

As to the longitude, we had no other way but to take with our quadrant ſome altitudes of the moon's lower limb:

Mine were theſe:

Alt.	Times by clock.			Apparent times.			
°	h	'	''	h	'	''	'''
37	9	12	53	9	8	30	54
36		17	25		13	2	43
35		21	55		17	32	31
34		26	21		21	58	20
33		30	48,5		26	25	58

Theſe altitudes taken with a quadrant of two French ſeet radius, 1′ 6″ muſt be added to each, to correct the error of the quadrant.

M. de Fleurieu's were :

Alt.		Times by clock.			Apparent times.		
°	′	h	′	″	h	′	″
34	45	9	22	28,5	9	18	05,80
34	15		24	41,5		20	18,74
33	45		26	56		22	33,19
32	45		31	23,5		27	0,57
32	15		33	36,5		29	13,51
31	45		36	51,5		31	28,46

Theſe altitudes were taken with an Engliſh quadrant of M. Siſſon's make, 16 inches radius. 8′ 34″ are to be added to each altitude to correct the error of the inſtrument, and for the ſemi-diameter of the wire.

On computing theſe altitudes by M. Clairaut's tables, corrected *nearly* by obſervations made at Paris the 30th of May and the 1ſt of June 1751, I find the longitude of Cape Francois, Weſt of the meridian of Paris, by my own altitudes, 4^h 58′ 8″, and by thoſe of M. de Fleurieu 4^h 58′ 20″.

I return to Venus: the time which we have noted for the total entry is that when we perceived a very ſlender thread of light between the limbs of the ☉ and ♀. I judged that the limbs were in contact, but a few ſeconds before that inſtant. At the exit of ♀ in 1761, the limbs, being not yet in contact, and even ſenſibly diſtant aſunder, I ſaw as it were a dark ſpot detach itſelf from Venus, and gain the limb of the Sun; at which inſtant I eſtimated the internal contact. Many have this year ſeen the ſame phænomenon at the total entry of Venus. I was in expectation of it; neither I or my aſſociates perceived any ſuch thing. In 1761 the Sun's limbs were moſt exquiſitely well defined; in 1769 they undulated, eſpecially at the beginning of the entry; at the total entry the undulation was conſiderably leſs, and notwithſtanding this undulation I believe our obſervation a good one. On comparing the duration of the tranſit obſerved at the Prince of Wales's Fort, with that of Father Hell, at Wardhus, I find, on a firſt calculus, which I believe at leaſt *nearly* exact, that the Sun's parallax is $9''.11$.

Auguſt 16, at St. Croix in Teneriffe, the firſt ſatellite emerged at $9^h\ 16'\ 5''$, apparent time.

I am, &c.

Paris, March 10, 1770.

Pingré.

XLI. *Obſervations of Immerſions and Emerſions of* Jupiter's *firſt Satellite, made at* Funchal, *in* Madeira, *with a reflecting Teleſcope of* 18 *Inches Focus, made by Mr.* Short. *The Time was found by taking equal Altitudes, with a Quadrant of* 12 *Inches radius, made by Mr.* Bird, *and with the help of a good Pendulum Clock made in London. By the late* Thomas Heberden, *M. D. F. R. S.*

Read Dec. 13, 1770.

THE latitude of the place of obſervation in Funchal, by a mean of ſeveral obſervations made with the ſame quadrant = 32° 33′ 35″.

Obſervations of Jupiter's firſt Satellite, made at Madeira.

	Magnif. power	Apparent time	Obſer. made at Greenwich with a reflector of ſix feet focus, power 100	Calculated in the Connoiſſance des temps.	Diff. of Mer. between Madeira and Greenwich
		Emerſions.			
1763		h ′ ″		h ′ ″	h ′ ″
Dec. 26	95	7 16 4		8 31 47	1 6 27
1764					
Jan. 11	95	5 27 35 hazy		6 42 55	1 6 4
25	95	9 12 42		10 28 24	1 6 26
Feb. 10	55	7 29 47		8 45 34	1 6 31
17	55	9 25 12 flying clouds		10 41 3	1 6 35
Mar. 27	55	8 9 3 clouds		9 24 48	1 6 29
		Immerſions.			
1765					
Dec. 24			10 39 27		
1766					
Jan. 16	55	9 31 20		10 47 48	1 7 12
Feb. 1	55	7 44 38		9 0 43	1 6 49
		Emerſions.			
Mar. 5	55	6 37 36 doubtful	7 43 55	7 53 28	1 6 36
				Calculated in the Naut. Alm.	
1767					
Apr. 16	55	8 9 48	9 16 13	9 16 55	1 7 7
May 9	55	8 25 35 flying clouds	9 32 26	9 33 12	1 7 37

Obſervations of Jupiter's firſt Satellite, made at Madeira.

	Magnif. power	Apparent time	Obſer. made at Greenwich with a reflector of ſix feet focus power 100	Calculated in the nautical almanac	Diff. of Mer. between Madeira and Greenwich
		Immerſions.			
768		h ′ ″		h ′ ″	h ′ ″
ar. 26	55	8 26 51		9 34 19	1 7 28
r. 2			11 29 23		
		Emerſions.			
r. 18			12 1 37		
25			13 57 19		
27	55	7 19 24		8 26 42	1 7 18
ay 4	55	9 15 0 doubtful		10 22 13	1 7 13
11			12 16 46		
18			14 13 17		
20	55	7 33 27		8 40 42	1 7 15
27	55	9 27 33		10 34 57	1 7 24
ne 3			12 28 6		
ly 5	55	7. 52 10 flying clouds		8 59 24	1 7 14
			Diff. of the two mer. by the obſ. of 16 April, 1767		
			1 6 25		
ld for diff. of teleſcopes - - - -			25		
diff. of meridians - - - -			1 6 50		1 6 $55\frac{10}{17}$ mean.

N. B. 25″ are to be allowed for the difference between the reflector uſed at Greenwich and at uſed at Madeira.

9′ 16″ are allowed for the difference of meridians between Paris and Greenwich.

XLII. Ac-

XLII. *Account of the Tranſit of* Mercury, *obſerved at* Norriton, *in* Pennſylvania, Nov. 9. 1769 *agreeable to an Appointment of the* American *Philoſophical Society, held at* Philadelphia, *for promoting uſeful Knowledge. By* William Smith, *D. D. Provoſt of the College of* Philadelphia; John Lukens *Eſq; Surveyor General of* Pennſylvania; David Rittenhouſe, *M. A. and Mr.* Owen Biddle. *Communicated by* Benjamin Franklin, *LL. D. F. R. S. and Preſident of the Philoſophical Society at* Philadelphia.

Read Dec. 13, 1770.

AS tranſits of Mercury are more frequent than thoſe of Venus, we need not be ſo particular in this account, as we were in that of Venus.

We had the ſame teleſcopes now as before, *viz.*

1. The college reflector, with Dollond's micrometer; uſed by myſelf, with a magnifying power of 200, to obſerve the *contacts*.
2. A refractor of 42 feet, magnifying 140 times, uſed by Mr. Lukens.

3. Mr. Rittenhouſe's refractor, with about the ſame power, uſed by himſelf.

Mr. Biddle had no teleſcope; but was very ſerviceable in the other parts of the obſervation.

Although there were many flying clouds, which frequently obſcured the Sun in the forenoon of the day; yet from about one o'clock till half an hour paſt three, the Sun ſhone perfectly clear, and undiſturbed by clouds; which gave us an opportunity, as favorable as we could wiſh, for obſerving the contacts, and making ſome micrometer meaſures.

The firſt external contact was obſerved to the ſame inſtant by all the three obſervers, who had no communication with each other, the two refractors being out of doors, and the reflector within the obſervatory; and the contacts noted (as at the tranſit of Venus) by ſignals given to perſons ſet at the windows of the obſervatory, to count the clock.

The contacts were as follows:

1769, Nov. 9, apparent time,

	h	′	″	
At	2	35	17	firſt external contact, by all the three obſervers.
	2	36	35	firſt internal contact, by Dr. Smith and Mr. Rittenhouſe.
	2	36	33	firſt internal contact, by Mr. Lukens.

	In.	20ths	500ths	′	″
☉'s diam. per microm.	3	13	7	32	20,24
Mercury's diam. taken backwards and forwards ſeveral times, and the ſum halved, gave only				☉	8,22

Nov. 9, 1769. apparent time.			Micrometer meaſures of leaſt diſtance of the neareſt limbs of ☉ and ☿.			Reduced to Minutes and Seconds of ☉'s Diameter.	
h	′	″	In.	20ths	500ths	′	″
3	1	44	0	5	14 doubtful	2	26,2
3	10	9	0	6	12	2	50,5
3	19	7	0	8	1	3	31,84
3	31	11	0	10	0	4	23,78

From 31 minutes paſt three, the ſun was conſtantly obſcured in a cloud, that deſcended with him, till at 32′ paſt 4 he broke out into a ſhort glimpſe of three minutes; during which the diameter of ☿ was again meaſured, and came out as before. One micrometer meaſure more was alſo now attempted of the neareſt diſtance of the limbs, but the ☉ got under a cloud before it was completed, to our great regret, as we wiſhed to have at leaſt one more meaſure at an hour's diſtance from the reſt. More might have been taken during the firſt half hour, after three; but thoſe that were taken are ſufficiently near each other, and any between them would have been uſeleſs for a projection, as thoſe we have may be depended on.

The following obſervations of another kind were all that Mr. Rittenhouſe could obtain, *viz.*

Apparent time.
h ′ ″

3 3 30 ☉'s lower limb at horizontal wire.
3 3 42 ☉'s preceding limb at vertical wire.
3 5 58 ☿'s center at vertical wire.
3 6 31 ☉'s ſubſequent limb at vertical wire.

3 6 32

h	′	″	
3	6	32	☿'s center at horizontal wire.
3	7	18	☉'s upper limb at horizontal wire.
—	—	—	— — — — —
4	30	34	☉'s lower limb at horizontal wire.
4	31	4	☉'s preceding limb at vertical.
4	32	39	☿'s center at vertical.
4	33	41	☉'s upper limb at horizontal.

The two remaining obſervations of this ſett could not be got, the ſun being again obſcured by a cloud, and appearing no more that day. They had ſomething more of the ſun at Philadelphia, and got ſome micrometer meaſures after four o'clock. By the contacts of mercury at Philadelphia and Norriton, we get the latter 55″ of time weſt of the ſtate-houſe obſervatory; the ſame we made by the eclipſes of Jupiter's ſatellites.

Philadelphia,
Dec. 19, 1769.

W. Smith.

XLIII. *A Letter to* James Weſt, *Eſq; Preſident of the Royal Society, containing the Inveſtigations of Twenty Caſes of Compound Intereſt, by* J. Robertſon, *Lib. R. S.*

S I R,

Read Dec. 13, 1770.

IT is well known that the conſideration of Compound Intereſt is of great utility in the Computations reſpecting the values of Penſions, Annuities, Reverſions, and other affairs relating to Money concerns; and therefore, many Mathematicians have beſtowed ſome time on this uſeful ſubject, and have endeavoured to diſcover practical methods for ſolving the various caſes that might occur: And in theſe inquiries, the uſe of Logarithms has been found of ſingular ſervice in facilitating the operations.

The late William Jones, Eſq; F. R. S. among the variety of Mathematical matters to which he gave attention, conſidered the buſineſs of Compound Intereſt fully, and did, many years ago, cauſe to be engraved on a Copper-plate, more caſes in Intereſt than had been exhibited before that time: Several copies of impreſſions from that plate were diſtributed among his friends; to whom it appeared that he had treated this ſubject in a more extenſive manner than had been done by other Mathematicians.

The Theorems, or Rules, for the Caſes of Compound Intereſt, without their inveſtigations, were inſerted by Mr. Jones, in the quarto edition of Logarithms, publiſhed by Gardiner; and the Rules were alſo communicated to Mr. Dodſon, who publiſhed them, by Mr. Jones's leave, with examples to illuſtrate the uſe of his Antilogarithmic Tables: But the inveſtigations of theſe Theorems not having yet been made public, it is apprehended that Gentlemen curious in theſe ſpeculations would be pleaſed to ſee them: And ſhould the following Eſſay be approved of by you, who have been for many years acquainted with the Finances of this Nation, and the importance of a ready knowledge in computing the Intereſt on the National Loans, you will be pleaſed to communicate it to the Royal Society.

I am, SIR,

Your moſt humble ſervant,

John Robertſon.

In this ſubject five particulars are taken into conſideration.

1ſt, The Annuity, Rent, or Penſion.

2d, The Times that Annuity, Rent, or Penſion is to continue.

3d, The Rate of Intereſt uſed in the computation.

4th, The Amount of thoſe Rents, and their Intereſt, when they are forborn to be received any times after they are due.

5th, The preſent worth of thoſe Rents, ſome times before they are due; or, of a Sum to be received before it is due, Diſcount being allowed.

And the inveſtigations naturally fall under two heads.

Firſt, The Conſideration of Amounts.

Secondly, The Conſideration of Diſcounts.

Under the firſt head an Equation is to be obtained between the Annuity, Time, Rate and Amount, from the known proportion that ſubſiſts between ſums of money put to intereſt, during the ſame length of time, and the amounts of the principal and intereſt together.

Under the ſecond head another Equation is to be formed between the Annuity, Rate, Time and preſent Worth, from the known proportion that ſubſiſts between the ſums diſcounted, and their preſent worths, when done for the ſame time.

As theſe Equations involve quantities common to both of them, therefore other Equations may be thence deduced, containing all the five terms before ſpecified. And hence, any three of the five terms being given, the other two are to be found, which admits of 20 Caſes.

Some of theſe Caſes will produce Adfected Equations, where the index of the higheſt power of the unknown quantity will be the number of times the Rent is to continue, or to be paid: Therefore, the ſolution of thoſe Caſes will be given by a method of Approximation, as no better way has yet been diſcovered for the ſolution of Adfected Equations, in numbers, above the third or fourth degree.

In the following inveſtigations,

Let a = Annuity, Rent, or Penſion.

n = Number of times that intereſt is to be paid for the annuity, or ſum lent.

r = Rate of intereſt of 1 £. for 1 time.

m = Amount of the annuity, or ſum lent for n times, at r intereſt.

p = Principal ſum uſed, or preſent worth of a ſum before it is due.

Of Compound Intereſt.

Firſt, In Amounts,

Let $q = 1 + r$ = Amount of 1 £. for 1 time.

Now, a = laſt year's amount.

And $1 : q :: a : aq$ = laſt but one year's amount.

$1 : q :: aq : aq^2$ = laſt but two year's amount.

$1 : q :: aq^2 : aq^3$ = laſt but 3.

And ſo on to aq^{n-1} = firſt year's amount.

Therefore $a + aq + aq^2 + aq^3$ &c. $+ aq^{n-1} = m$.

But $a : aq :: m - aq^{n-1} : m - a$. Euc. 12. v.

Then $m - a = mq - aq^n$

Or $m - a = m + mr - aq^n$

Therefore $aq^n - a = mr$ Put $A = q^n$.

Then $\overline{A - 1} \times a = mr$

Therefore $A = \frac{mr + a}{a}$

Hence $a = \frac{mr}{A - 1}$

$m = \frac{\overline{A - 1} \times a}{r}$

$r = \frac{\overline{A - 1} \times a}{m}$.

Second, In Diſcounts.

Since $q : 1 :: a : \frac{a}{q} =$ 1ſt year's preſent worth.

$q : 1 :: \frac{a}{q} : \frac{a}{q^2} =$ 2d year's preſent worth.

$q : 1 :: \frac{a}{q^2} : \frac{a}{q^3} =$ 3d.

And ſo on to $\frac{a}{q^n} =$ nth years preſent worth.

Therefore $\frac{a}{q} + \frac{a}{q^2} + \frac{a}{q^3} +$, &c. to $\frac{a}{q^n} = p$.

But $\frac{a}{q} : \frac{a}{q^2} :: p - \frac{a}{q^n} : p - \frac{a}{q}$. Euc. 12. v.

Therefore $\frac{pq^n - a}{q^n} = pq - a$.

Or $\frac{pq^n - a}{q^n} = p + pr - a$.

Therefore $aq^n - a = prq^n$.

Or $\overline{A - 1} \times a = prA$.

Therefore $A = \frac{a}{a - rp}$

Hence $\overline{A - 1} \times a = mr = prA$.

Therefore $A = \frac{mr + a}{a} = \frac{a}{a - rp} = \frac{m}{p} = \overline{1 + r}^n$.

Caſe I. Given a, m, p: Required r.

Since $A = \frac{m}{p}$.

Therefore $r = \frac{\overline{A - 1} \times a}{m}$.

Caſe II. Given p, m, n: Required r.

Since $\overline{r+1}|^{n} = \frac{m}{p}$.

Therefore $r = \overline{\frac{m}{p}}|^{\frac{1}{n}} - 1$.

Caſe III. Given a, m, n: Required r.

Since $\overline{A-1} \times a = mr$.

Therefore $\left(\frac{A-1}{nr} =\right) \frac{m}{na} = \frac{\overline{1+r}|^{n} - 1}{nr}$.

Now $\overline{1+r}|^{n} = 1 + nr + n \times \frac{n-1}{2} r^{2} + n \times \frac{n-1}{2} \times \frac{n-2}{3} r^{3}$, &c.

Therefore $\frac{m}{na} = 1 + \frac{n-1}{2} r + \frac{n-1}{2} \times \frac{n-2}{3} r^{2}$, &c.

And $\overline{\frac{m}{an}}|^{\frac{2}{n-1}} = \overline{1 + \frac{n-1}{2} r + \frac{n-1}{2} \times \frac{n-2}{3} r^{2}}|^{\frac{2}{n-1}}$; which, by the Binomial Theorem, will become $= 1 + r + \frac{n+1}{12} r^{2}$ (nearly).

Let $D = \left(\overline{\frac{m}{na}}|^{\frac{2}{n-1}} =\right) 1 + r + \frac{n+1}{12} r^{2}$,

Then $r^{2} + \frac{12}{n+1} r = \overline{D-1} \times \frac{12}{n+1}$, Let $2E = \frac{12}{n+1}$,

Then $r + E = \left(\sqrt{2 \times \overline{D-1} + E \times E} =\right) F$.

Therefore $r = F - E$.

In this Solution, 1ſt, find $D = \overline{\frac{m}{na}}|^{\frac{2}{n-1}}$,

2d, find $E = \frac{6}{n+1}$,

3d, find $F = \sqrt{2 \times \overline{D-1} + E \times E}$,

4th, find $r = F - E$.

Caſe IV. Given a, p, n; Required r.

Since $\overline{A-1} \times a = Apr$.

Therefore $\frac{p}{na} = \frac{A-1}{Anr} = \frac{1-A^{-1}}{nr} = \frac{1-\overline{1+r}|^{-n}}{nr}$.

Now, $\overline{1+r}|^{-n} = 1 - nr + n \times \frac{n+1}{2} r^2 - n \times \frac{n+1}{2} \times \frac{n+2}{3} r^3 +$ &c,

Therefore $\frac{p}{na} = 1 - \frac{n+1}{2} r + \frac{n+1}{2} \times \frac{n+2}{3} r^2$, nearly.

Then $\overline{\frac{p}{na}}|^{-\frac{2}{n+1}} = \overline{1 - \frac{n+1}{2} r + \frac{n+1}{2} \times \frac{n+2}{3} r^2}|^{-\frac{2}{n+1}}$; which, by the Binomial Theorem, will become

$$= 1 + r - \frac{n-1}{12} rr, \text{ nearly.}$$

Now, $\overline{\frac{p}{na}}|^{-\frac{2}{n+1}} = \overline{\frac{na}{p}}|^{\frac{2}{n+1}}$.

Let $G = \left(\overline{\frac{na}{p}}|^{\frac{2}{n+1}} =\right) 1 + r - \frac{n-1}{12} r^2$.

Therefore $rr - \frac{12}{n-1} r = - \overline{G-1} \times \frac{12}{n-1}$.

Let $2H = \frac{12}{n-1}$.

Then $r - H = \left(\sqrt{\overline{H-2.\overline{G-1}} \times H} =\right) K$.

Therefore $r = H - K$.

Caſe V. Given a, n, r; Required m.

Since $\overline{A-1} \times a = mr$. Therefore $m = \frac{\overline{A-1} \times a}{r}$.

Caſe VI. Given p, n, r; Required m.

Since $A = \frac{m}{p}$. Therefore $m = pA$.

Caſe VII. Given a, p, r; Required m.

Since $\frac{m}{p} = \frac{a}{a - rp}$. Therefore $m = \frac{ap}{a - rp}$.

Caſe VIII. Given a, p, n; Required m.

Find $G = \overline{\frac{na}{p}}|^{\frac{2}{n+1}}$; $H = \frac{3}{\frac{1}{2}n - 1}$; $K = \sqrt{\overline{H - 2\overline{G - 1}} \times H}$.

Now, $H - K = r$ by 4th. But $\overline{r + 1}|^{n} = \frac{m}{p}$.

Therefore $m = \overline{r + 1}|^{n} \times p$.

Caſe IX. Given a, n, r; Required p.

Since $\overline{A - 1} \times a = prA$. Therefore $p = \frac{\overline{A - 1} \times a}{Ar}$.

Caſe X. Given m, n, r; Required p.

Since $A = \frac{m}{p}$ .Therefore $p = \frac{m}{A}$.

Caſe XI. Given a, m, r; Required p.

Since $\frac{mr + a}{a} = \frac{m}{p}$. Therefore $p = \frac{ma}{mr + a}$.

Caſe XII. Given a, m, n; Required p.

Find $D=\overline{\frac{m}{na}}\Big|^{\frac{2}{n-1}}$; $E=\frac{3}{\overline{\frac{1}{2}n+1}}$; $F=\sqrt{\overline{2\overline{D-1}+E}\times E}$

Now, $F-E=r$, by 3d. But $\overline{1+r}|^{n}=\frac{m}{p}$.

Therefore $p=\frac{m}{\overline{1+r}|^{n}}$.

Caſe XIII. Given p, n, r; Required a.

Since $\overline{A-1}\times a=prA$. Therefore $a=\frac{prA}{A-1}$.

Caſe XIV. Given p, m, n; Required a.

Since $A=\overline{r+1}|^{n}=\frac{m}{p}$. Therefore $r+1=\overline{\frac{m}{p}}\Big|^{\frac{1}{n}}$.

Hence $A-1$, and $B-1$ $(=r)$ are known.

Therefore $a=\frac{m\times\overline{B-1}}{A-1}$.

Caſe XV. Given m, n, r; Required a.

Since $\overline{A-1}\times a=mr$. Therefore $a=\frac{mr}{A-1}$.

Caſe XVI. Given p, m, r; Required a.

Since $A=\frac{m}{p}$, $A-1$ is given.

Therefore $a=\frac{mr}{A-1}$.

Caſe XVII. Given m, p, r; Required n.

Put L, for logarithm; and $\grave{\mathrm{L}}$, for arith. comp. of a log.

Since $\overline{1+r}|^n = \frac{m}{p}$. Therefore $n = \frac{\mathrm{L},\, m + \grave{\mathrm{L}},\, p}{\mathrm{L},\, \overline{r+1}}$.

Caſe XVIII. Given a, p, r; Required n.

Since $\overline{1+r}|^n = \frac{a}{a-pr}$. Therefore $n = \frac{\mathrm{L},\, a - \mathrm{L},\, \overline{a-pr}}{\mathrm{L},\, \overline{r+1}}$.

Caſe XIX. Given a, p, m; Required n.

Since $\mathrm{A} = \left(\overline{r+1}|^n =\right) \frac{m}{p}$,

Then $\left(\mathrm{L},\, \overline{r+1}|^n =\right) \mathrm{L},\, m - \mathrm{L},\, p = \mathrm{L},\, \mathrm{A}$;

Hence A, and A — 1, are known.

Alſo $\mathrm{L},\, \overline{\mathrm{A}-1} + \mathrm{L},\, a + \grave{\mathrm{L}},\, m = \mathrm{L},\, \overline{\mathrm{B}-1}$ (by Caſe 14th):

Hence $r = (\mathrm{B}-1)$, and $r+1$, are known.

Therefore $n = \frac{\mathrm{L},\, \mathrm{A}}{\mathrm{L},\, \overline{r+1}}$.

Caſe XX. Given a, m, r; Required n.

Since $(\mathrm{A} =) \frac{mr+a}{a} = \overline{1+r}|^n$.

Therefore $n = \frac{\mathrm{L},\, \overline{mr+a} - \mathrm{L},\, a}{\mathrm{L},\, \overline{1+r}}$.

XLIV. *A Copy of a Letter from* John Ellis, *Eſq; F. R. S. to Dr.* Linnæus, *F. R. S.* &c. *with the Figure and Characters of that elegant* American *Evergreen-tree, called by the Gardiners the* Loblolly-Bay, *taken from Bloſſoms blown near* London, *and ſhewing that it is not an* Hibiſcus, *as Mr.* Miller *calls it; nor an* Hypericum, *as Dr.* Linnæus *ſuppoſes it; but an intire new Genus, to which Mr.* Ellis *gives the Name of* Gordonia.

DEAR SIR,

Read Dec. 20, 1770.

YOU are no ſtranger to the frequent errors that ariſe in botany, by taking the characters of new genera from ſpecimens of dried plants; at the ſame time it is well known to be an eſtabliſhed rule with you to alter or amend any thing of this kind, as ſoon as it appears clear to you from better evidence. This is the reaſon that I addreſs you with more freedom in the preſent botanical cauſe, in which you are concerned.

You muſt know then, that we have lately got into a method of cultivating that elegant evergreen-tree, called in South Carolina and the Floridas, the Loblolly-bay, or *Alcea Floridana*, *&c.* of Cateſby's Hiſtory of Carolina, vol. I. tab. 44. p. 44.

This tree has lately produced ſome well-blown flowers, in the curious botanic garden of Mr. Bewick, at Clapham, near London, who was ſo obliging to ſend them to me to examine their characters while freſh: I had very fortunately by me ſome dried ſpecimens, which had been ſent me from our mutual friend Dr. Alexander Garden, of Charles-town, South-Carolina. When I compared the freſh ſpecimens with the dried ones, it ſoon became evident to me, why you judged it to be of the claſs of Polyadelphia, and placed it among the *Hypericums* with the trivial name of *Laſianthus*. For the ſtamina in the dried ſpecimen appeared to be divided into five diſtinct phalanges, or bundles, with their filaments united together; but when you obſerve the figure and deſcription of the new-blown flower, you will find that the filaments of the ſtamina being united at the bottom in a circle round the top of a funnel-ſhaped tube, will bring it to the claſs of Monadelphia, and probably next to the *Stewartia*. The only doubt I have in the deſcription is, whether the ſtyle ſhould be called one or five, the latter of which numbers you have adopted, and perhaps more properly, but in that I ſhall ſubmit to your deciſion.

If, after you have ſeen it, you think, with many of your friends, both here and in America, that it is a new genus, I deſire it may have a place among your genera, by the name of *Gordonia*, as a compliment to our worthy friend, that eminent gardener Mr. James Gordon, near Mile-end, to whom the ſcience of botany is highly indebted, and whoſe merit is univerſally known for his great knowledge in the cultivation of exotic plants.

Mr. Miller, after telling us in his Gardener's Dictionary of the difficulty, or rather impoſſibility, to raiſe it, has placed it under the genus of *Hibiſcus*; but as both the characters of that genus, in which he has followed you, as well as the face and habit of this plant, differ ſo much from an *Hibiſcus*, I am convinced you will agree with me, that it does not belong to it.

Botanical Characters of the GORDONIA.

MONADELPHIA POLYANDRIA.

CALYX. The perianthium, or flower-cup, conſiſts of five concave, roundiſh, downy, and ſtiff leaves, hairy on their edges, and, upon the falling off of the corolla, embracing the germen or ſeed bud very ſtrongly: theſe remain till after the ſeed-veſſel is ripe.

Obſ. Upon the ſtalk under this flower-cup are four floral leaves, or bracteæ, placed at unequal diſtances, of an oblong form, concave, and roundiſh at top, and truncated at bottom, where each ſeems to embrace a part of the ſtalk. Theſe cover the flower, and its proper

per calyx, or flower cup, in its younger ſtate, but as it grows up and expands, they begin to appear more diſtant from each other, and ſoon decay; otherwiſe they might be ſuppoſed to have the appearance of an exterior flower cup.

COROLLA. The flower conſiſts of five large, fleſhy, concave, inverted-oval petals, united at the narrow part of the baſe, forming as it were one petal; in the inſide of this narrow part, or joining, is a funnel-ſhaped fleſhy ſubſtance, like a nectarium; which is united to, and appears to be, a part of the petal; this ſurrounds the germen, or embryo ſeed-veſſel. The upper part of this fleſhy funnel-ſhaped tube is waved in ſuch a manner, that the riſing of each wave anſwers to the middle of each petal.

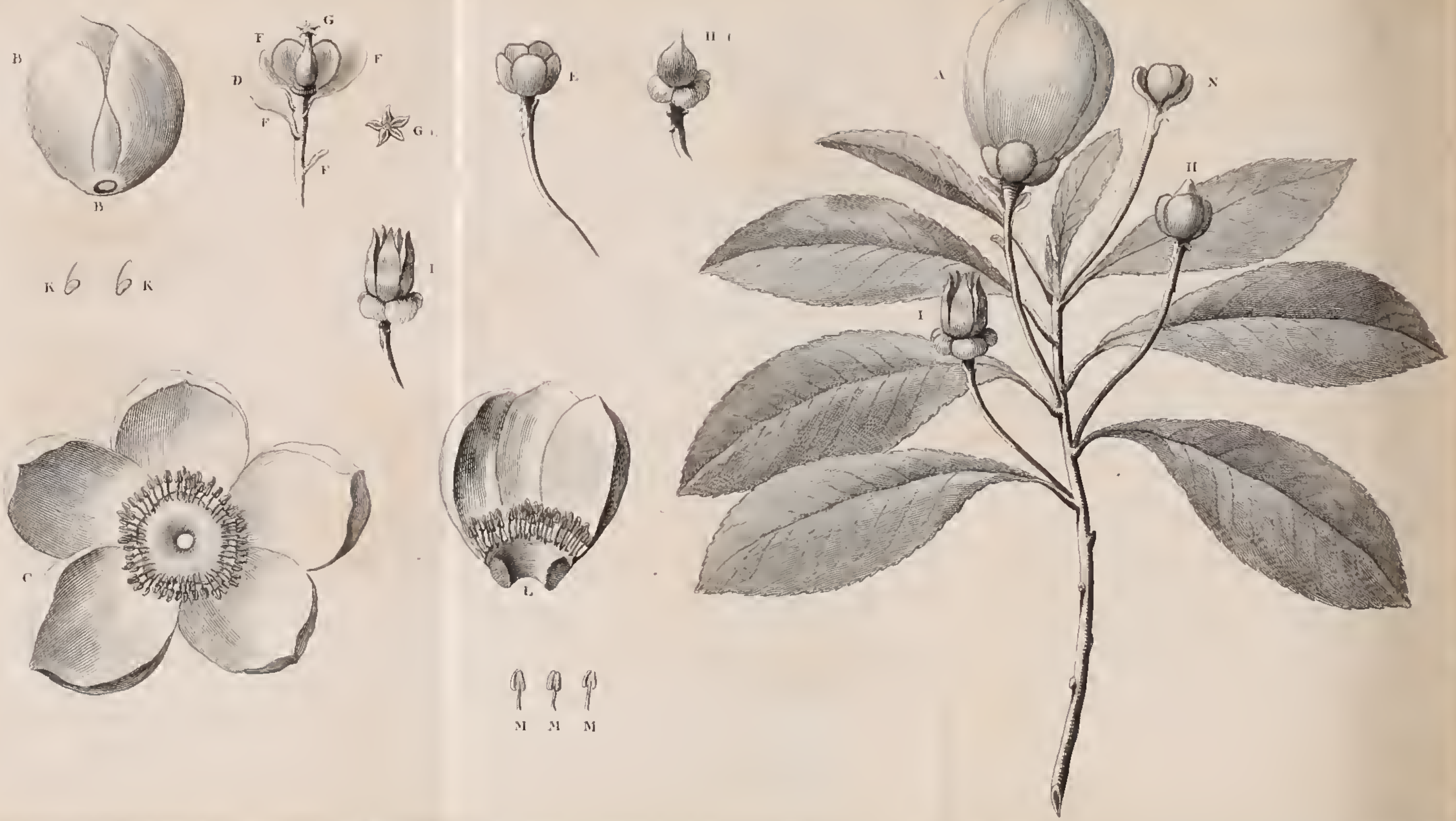
STAMINA (or male organs). The filaments of theſe are numerous, and linear ſhaped, they are inſerted all round on the top or margin of this waved part of the funnel-ſhaped tube (not ſeparated by diſtinct parcels, or united only in different bundles, as in the claſs of Polyadelphia) but equally diſtinct from each other, and connected together at the bottom by this fleſhy tube. The antheræ, or ſummits, that contain the male-duſt, are of an oval form and erect; they have a cell on each ſide full of farina, of a globular ſhape, when viewed by the microſcope, different from the malvaceous tribe, which are full of points, and a common object in microſcopes.

PISTILLUM (or female organ). The germen, or ſeed-bud, is oval, and acuminated towards the top, where the ſtyle begins. The ſtyle is very ſhort, and has five little ridges on the ſides, as if five ſtyles were united together. The tops of theſe ſuppoſed connected ſtyles, end in five acute ſtigmata, or openings of the ſtyle, diſpoſed horizontally in a radiated form, a little contorted towards the points. On the upper part of each of theſe is a longitudinal cavity, covered with down, and ending with each of them in a point.

PERICARPIUM. The capſule is eggſhaped and acuminated at the top. It is of a woody conſiſtence, and ſplits open at top when ripe into five valves, with five cells, or loculaments.

SEMINA. The ſeeds are kidneyſhaped, and winged obliquely on one ſide; there are two ſeeds in each cell.

Gordonia Lasianthus. *Vulgo* Loblolly Bay.



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Explanation of PLATE XI.

A The flower of the *Gordonia Laſianthus*, unopened, with its calyx and bracteal leaves; from the curious garden of Benjamin Bewick, Eſq; at Clapham.

B The petals dropt off, theſe are united at their baſe; ſee B 1.

C The petals, or corolla expanded, to ſhew the fleſhy ſunnel-ſhaped tube, which unites the filaments together at their baſe.

D The piſtillum, whoſe germen, or ſeed-bud, has been ſurrounded by the baſe of the corolla at B 1.

E The calyx, or flower-cup, conſiſting of five little ſtiff leaves.

FFFF The four bracteal leaves.

G The ſhort ſtyle, and five ſtigmata.

G 1 The ſtigmata magnified.

H The conical germen, or ſeed bud, ſurrounded by the calyx.

H 1 The ſeed veſſel before it is ripe, with the calyx reflexed and withered.

I The pericarpium, or ſeed-veſſel, with its valves open.

K K Two winged ſeeds.

L Three of the petals cut off, to ſhew how they are united to the fleſhy funnel-ſhaped tube, that ſupports the ſtamina.

M M M The ſtamina with their filaments and ſummits a little magnified.

N The bracteal leaves ſurrounding the flower-bud unopened.

XLV. *The Copy of a Letter from* John Ellis, *Eſq; F.R.S. to Mr.* William Aiton, *botanic Gardener to her Royal Highneſs the Princeſs Dowager of* Wales, *at* Kew, *on a new Species of* Illicium Linnæi, *or Starry Aniſeed Tree, lately diſcovered in* Weſt Florida.

SIR,

Read Dec. 13, 1770.

YOUR extenſive knowledge in botany, and great ſkill in the cultivation of the rareſt exotic plants from all parts of the world in the celebrated botanic garden of her royal highneſs the princeſs dowager of Wales at Kew, will I hope, apologize for me to the Royal Society for the liberty I take in laying this letter, which I addreſs to you, before them; eſpecially as moſt of that illuſtrious body, who have made that agreeable ſcience their ſtudy, are fully convinced of the truth of it, by often having the pleaſure of ſeeing that moſt noble and truly princely collection, which is ſo happily placed under your care.

I confeſs myſelf particularly obliged by your ſkill and attention in raiſing not only the oak acorns, which were put into your hands by the Royal Society, after they had been preſerved by me a whole ſeaſon in wax; but alſo for the care you have taken in raiſing one of the firſt tea-plants that has been produced from ſeed in this kingdom; eſpecially as it came to me in ſuch a ſituation, that it could ſcarcely be expected to vegetate.

You have further ſhewed your ſkill in raiſing many of the ſtarry aniſeed trees from Weſt Florida after they had been confined in a box under the ſhip's decks for near four months, a voyage near as long as from St. Helena, or even the Cape of Good Hope.

I ſhall now proceed to give you a hiſtory of this curious tree, both as a native of Japan, China, and other parts of the Eaſt, as well as both the Floridas in North America.

We meet with an account of the Eaſtern one, together with a figure of it, taken from Cluſius, in Parkinſon's Theatre of Plants, *p.* 1569. where he obſerves, that ſome branches of it, with the huſks and ſeeds only, without leaves or bloſſoms, were brought into England by Sir Thomas Cavendiſh, in queen Elizabeth's time, from the Philippine Iſlands, where he met with it in his voyage round the world. Theſe branches were given to Mr. Morgan, the queen's apothecary, and to Mr. James Garrat, of whom Cluſius received them.

Monſieur Geoffroy, in his Materia Medica, tranſlated in 1736 by Dr. G. Douglaſs, *p.* 322. calls it *Aniſum Sinenſe*, *Semen Badian*, & *Fructus Stellatus*, and ſays, it is highly eſteemed in China, and all over the Eaſt. That it is uſed to cure any bad taſte in the mouth, as a preſervative againſt the effects of bad air, and alſo for the ſtone and gravel. The Indians likewiſe ſteep this fruit in water, and afterwards ferment the infuſion, and thus make a vinous liquor: that the Dutch in the Eaſt-Indies, as well as the natives, mix this fruit with their tea and ſherbet.

Kæmpfer in his Amœnitates Exoticæ, *p*. 880. calls it *Somo*, or *Skimmi*, and has given us a very good figure of a branch of it, with the leaves, flowers, and fruit. He found it in Japan, and ſays that the Japoneſe and Chineſe eſteem it a ſacred tree, that they offer it to their idols, and burn the bark of it, as a perfume, on their altars; and lay the branches upon the graves of the dead, as an offering to the ghoſts of their pious departed friends; and that the public watchmen uſe the powder of this aromatic bark ſtrewed in ſmall winding groves, or little channels, on ſome aſhes in a box ſecured from the weather, for the following purpoſe. This powder being lighted at one end, burns ſlowly on, and being come to certain marked diſtances, they ſtrike a bell, and by means of this time-keeper, proclaim the hours of the night to the public. And laſtly, that it has the remarkable property of rendring the poiſon of the bladder-fiſh (*Tetraodon Ocellatus* of Linn. Syſt. of Nature, *p*. 333.) more virulent, as many have experienced, that have uſed violent means to deſtroy themſelves.

We are indebted for the firſt diſcovery of this curious American tree to a negro ſervant of William Clifton, Eſq; chief juſtice of Weſt Florida, who was ſent to collect ſpecimens of all the rarer plants by his maſter, at my requeſt; and in April, 1765, he met with this curious tree growing in a ſwamp near the town of Penſacola; the ſpecimens I received in July following.

After this, in the latter end of January, 1766, Mr. John Bartram, the king's botaniſt for the Floridas, diſcovered it on the banks of the river St. John, in Eaſt Florida, as appears from his deſcription of it, and the drawing of a ſeed-veſſel, with ſome of the leaves, which he ſent to our late worthy member Peter Collinſon, Eſq; who was ſo kind as to communicate them to me. Mr. Bartram's deſcription of it, as it appears in his journal up the river St. John's, publiſhed by Dr. Stork, in his account of Eaſt Florida, is as follows:

" Near here my ſon found a lovely ſweet tree, " with leaves like the ſweet bay, which ſmelled like " ſaſſafras, and produces a very ſtrange kind of ſeed- " pop; but all the ſeed was ſhed, *the ſevere froſt* " had not hurt it, ſome of them grew near twenty " feet high, a charming bright evergreen-aro- " matic."

This obſervation of Mr. Bartram, relating to its bearing a ſevere froſt, may afford us a uſeful hint in the cultivation of this tree, eſpecially as I am convinced, from repeated accounts of the weather in Weſt Florida; that the froſt is much more intenſe there, from whence thoſe plants, which you now have in vigour, were brought, than in Eaſt-Florida; ſo that the experiment is well worth making with one of them, to ſee how far it will ſtand the ſeverity of our winters. Should it ſucceed, it would be a very great acquiſition to our gardeners, and be highly ornamental to our plantations of evergreens.

The medicinal properties of this tree are certainly worth enquiring into. The leaves afford a moſt agreeable bitter. A ſprig of it ſet to putrify in a phial of water, the bark ſoon became full of a clear mucilage. The young bloſſoms put into water with a ſmall quantity of oil of tartar *per deliquium* from a dark reddiſh color, became a light brown; but from the ſame proportion of oil of vitriol in water, they turned to a fine carmine color, which ſtained the paper of a fine red. This points out its aſtringent quality.

Before I come to the botanical characters of our Florida *Illicium*, I muſt obſerve, that it appears to me to be a *different ſpecies* from the oriental one.

The ſeed veſſels from China, which are to be ſeen in collections of the Materia Medica, eſpecially among foreigners, ſmell very diſagreeably of aniſeed: our Florida ſeed veſſel is agreeably aromatic, as are the leaves and young branches. The flower, according to Kæmpfer, is of a yellowiſh white, and looks at a diſtance like a Narciſſus: ours is of a dark red color.

Kæmpfer reckons the number of petals ſixteen, and the rays or ſeed-veſſels eight: the number of petals in ours is from twenty-one to twenty-ſeven, and the ſeed-veſſels twelve or thirteen that ripen. In reſpect to the form and growth of the tree, they are much the ſame; for inſtance, they both grow to the ſize of a cherry-tree; their leaves are of an oblong oval ſhape, pointed at both ends, fleſhy, with few veins, growing alternately, and in tufts at the ends of the ſmall branches.

Dr. Linnæus, who takes his characters of the *Illicium Aniſatum* (Gen. Plant. *p.* 244.) from Kæmpfer, places it among the Dodecandria Polygynia. But I am perſuaded you will agree with me, that from the following characters, ours muſt be of the Polyandria Polygynia, and ſhould ſtand next to the *Magnolia.*

Characters of the ILLICIUM FLORIDANUM, Or FLORIDA STARRY ANISEED TREE.

CALYX. The perianthium, or flower-cup, conſiſts commonly of five little membranaceous and colored leaves, that ſoon fall off; they are of a concave oblong oval form, pointed at the ends. Sometimes we meet with only four little leaves, ſometimes ſix, in the flower-cup. Kæmpfer obſerved four in his.

COROLLA. The flower conſiſts of many petals (from twenty-one to twenty-ſeven) which are lanceolated: theſe are of three ſizes, and equal numbers in each circle, the outward ones are long (about an inch) concave, obtuſe, and ſpreading open. The next are a little ſhorter and narrower; and the innermoſt are ſtill ſhorter, much narrower, and very ſharp pointed; but are not nectaria, as Dr. Linnæus ſuppoſes.

STAMINA. The filaments are many, (about thirty) very ſhort and flat, placed over one another, ſurrounding the germina, or embryo ſeed veſſels. Theſe ſupport as many antheræ, or ſummits, which are erect, oblong, and emarginated, or having a ſmall indenture at top, with a cell on each ſide full of farina, of a globular form when they are magnified.

Pistillum, or female organ. The germina, or embryo ſeed-veſſels, are twenty, or more, in number, placed in a circular order above the receptacle of the flower; they are compreſſed, erect, and ending in ſo many ſharp pointed ſtyles, bending outwards at the top. The ſtigmata, or openings on the top of the ſtyles, are downy, and placed lengthways along the upper part of each ſtyle.

Pericarpium, or ſeed-veſſel, conſiſts of twelve, oftner thirteen, little pods, or capſules, that ripen. Theſe are of a compreſſed oval ſhape, and a hard leather-like ſubſtance, with two valves to each, and are diſpoſed edge-ways in a circular order, like ſo many rays of a ſtar.

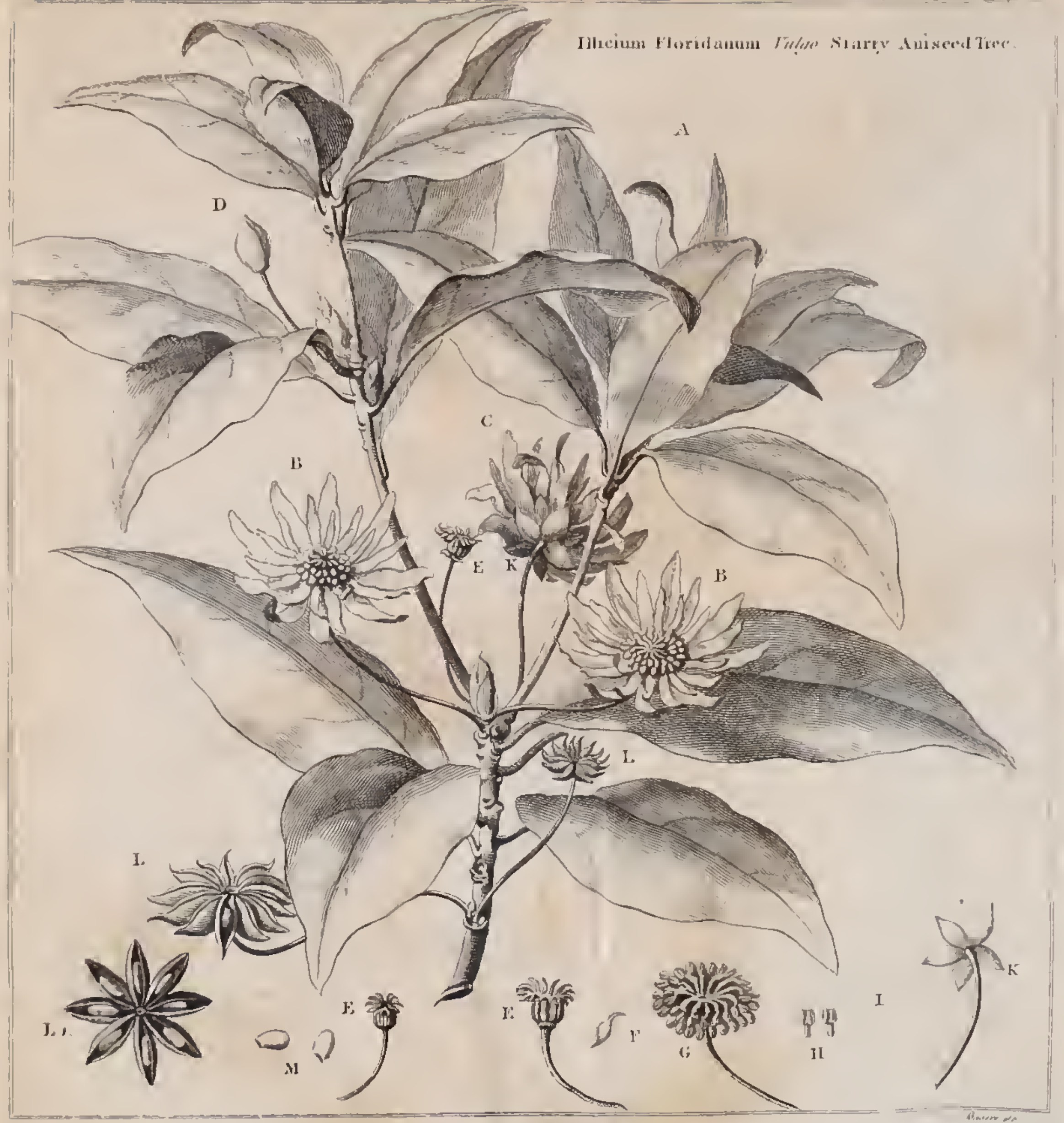
Semina. The ſeeds are ſmooth and ſhining, of an oval ſhape, a little compreſſed, and appear obliquely cut off at the baſe. There is one ſeed in each capſule. I am,

Sir,

Your affectionate friend,

John Ellis.

Illicium Floridanum *Vulgo* Starry Aniseed Tree.



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EXPLANATION of PLATE XII.

A A branch of the Illicium Floridanum, drawn from a plant in her Royal Highneſs the Princeſs Dowager of Wales's garden at Kew. The flowers and ſeed-veſſels were drawn from ſpecimens ſent over from Penſacola by his honour lieutenant governor Durnford.

BB The front view of two flowers.

C The back view of a flower.

D The bud of a flower unopened.

E E E The piſtilla, or female organs, being the embryo ſeed-veſſel, ſeparated from the ſtamina, or male organs.

F One ſingle piſtillum, with the germen, ſtyle, and ſtigma.

G The male and female organs, a little magnified.

H Two ſtamina, a little magnified.

I The farina fœcundans or male duſt.

K K The calyx, with five little leaves.

L L The ſeed-veſſels, with thirteen capſules.

L l The ſeed-veſſel of the Chineſe *Illicium*, with only eight capſules. Kæmpfer reckons the ſame number in the Japoneſe *Illicium*, which he calls *Somo*, or *Skimmi*.

M Two of the ſeeds; they are called ſemen badian, and uſed in medicine in Germany, Denmark, and Sweden. The Dutch import large quantities of them from China.

XLVI. *An Account of a very remarkable Meteor ſeen at* Oxford. *In a Letter to* Mathew Maty, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Cuſtos Archivorum of the Univerſity of* Oxford, *Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

Good Sir,

Read Dec. 20, 1770.

THE perſon that firſt ſaw the very remarkable luminous appearances in the air here, on Tueſday, October 24, 1769, as far as I have hitherto been able to learn, was the Rev. Mr. Cleaver, ſtudent of Chriſt-Church; who, on his return home, at a village called Horton, ſix or ſeven miles from Oxford, about 7ʰ 15′ P. M. obſerved, with ſome degree of aſtoniſhment, a dark fuſcous vapour, reſembling a blackiſh cloud, contiguous to the northern horizon. Out of this vapour there iſſued another of a flame colour, in the N.N.W. not unlike the meteor ſeen by me, Sept. 9, 1769, and deſcribed in the laſt volume of the *Philoſophical Tranſactions**. His account of it, if I remember right, was, that "it looked like an houſe, or building, ſet on fire." This at firſt was confined to a very ſmall ſpace in the heavens, but ſoon after expanded itſelf in ſuch a manner, that it covered a very large and extenſive tract in that part of the hemiſphere where it firſt appeared. In this ſtate the meteor continued till 7ʰ 45′ P. M. when it aſſumed a deep blood-red colour, moving a little towards the weſt, which gave it a very awful aſpect. Mr. Cleaver imparted to me no particulars relative to its extinction, but only ſaid, that he ſaw not the fainteſt traces of it after eight o'clock, ſo that it might probably about that time, or a little before, have totally diſappeared.

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* *Philoſoph. Tranſact.* Vol. LIX. for the Year 1769, p. 369, 370, 371, 372.

The ſame night, at 8ʰ 10′ P. M. I ſaw in the great quadrangle of Chriſt-church, and that part of Fiſh-Street adjoining to it, ſeveral lucid ſtreamers, aſcending in the N. and N. W. from the horizon, or rather a duſky kind of vapour contiguous to it, to a very conſiderable height. Theſe all moved towards the S. and S. E. with great velocity; and ſoon after many other ſimilar ſtreams of light ſhot up from the horizon, in various parts of the hemiſphere, particularly in the S. and S. E. They were all of a very pale yellow colour, ſuch as thoſe that form the *Auroræ Boreales* of the common kind. They conſtantly multiplied, in ſo amazing a manner, and with ſuch ſurprizing celerity, that by 8ʰ 15′ P. M. they ſeemed to have almoſt intirely covered the greateſt part of the hemiſphere, and then centered in a point a little to the S. of the zenith. They were attended by an infinite number of flaſhes, or coruſcations, and undulations of the lucid matter, as * is uſual in ſuch phænomena. In fine, the whole atmoſphere, or rather the whole collection of the luminous vapour lodged in it, was in a continual agitation for above a quarter of an hour; during which time, the whole hemiſphere ſeemed to be all on a blaze.

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* *Philoſoph. Tranſact.* ubi ſup. p. 367, 473. & alib.

This moſt glorious and extraordinary appearance was, however, of a very ſhort and inconſiderable duration; the extinction of the whole being ſo completely effected by $8^h\ 40'$ P. M. that no remains of the phænomenon, in any part of the heavens, could then be diſcerned.

But what principally engaged my attention this evening, was a luminous arch, or zone, of a very beautiful purple colour, ſuch as I had never ſeen before; which preſented itſelf to my view about $8^h\ 40'$ P. M. and extended from E. to W. nearly, as I apprehended, biſecting the hemiſphere. This began to grow faint a little before nine o'clock; and in leſs than ten minutes time, as near as I could gueſs, totally diſappeared.

The light caſt by the *Auroræ Boreales* above mentioned was greater than any I had ever obſerved to attend ſuch phænomena before. Nor, indeed, did I ever meet with a deſcription of any meteors reſembling that mentioned here in every particular. The converſion of the flame-colour in the firſt ſtage of the meteor into a deep blood-red, together with its wonderful expanſion, and the beautiful purple zone, or coloured arch, which cloſed the whole, are ſingularities that, I believe, never occured before; or, at leaſt, ſuch as have never hitherto met with a proper and adequate deſcription. Some of theſe phænomena ſeem, from the public papers, to have been ſeen at London, Windſor, and other places at a conſiderable diſtance from Oxford, about the ſame time that they appeared there; which remarkable circumſtance, on ſeveral accounts, merits a place in this letter. The whole city, for a ſhort time, ſeemed to be perfectly illuminated; the light caſt by the *Auroræ* ſucceeding the luminous appearance of a deep blood-red colour, being, as I apprehended, much ſuperior to that of the full moon. In fine, the whole phænomenon (or rather all the phænomena) was ſo very ſtriking and remarkable, that it was one of the moſt common topics of converſation, amongſt all orders and degrees of people here, for above a month after it appeared. I ſhall only beg leave to add, that the Reverend and learned Dr. Sharp, of Chriſt-Church, profeſſor of Greek in this univerſity, aſſured me that he ſaw ſome of the common *Auroræ Boreales* between eleven and twelve o'clock the ſame night; and that I have the honour to be,

GOOD SIR,

Your much obliged,

and moſt obedient,

humble ſervant,

Chriſt-Church, Oxon.
Sept. 27, 1770.

John Swinton.

XLVII. *An*

XLVII. *A Letter from* Richard Price, *D.D. F.R.S. to* Benjamin Franklin, *LL.D. F.R.S. on the Effect of the Aberration of Light on the Time of a Tranſit of* Venus *over the* Sun.

Dear Sir,

Read Dec. 20. 1770.

I Cannot doubt but that the obſervation made by your ingenious friend in the paper * you ſent me is right. The aberration of Venus muſt, I think, affect the phaſes of a tranſit, by retarding them, and not by accelerating them. This retardation is $55\frac{1}{2}''$; for that is the time nearly which Venus, during a tranſit, takes to move over $3''.7$. This, however, is by no means the whole retardation of a tranſit occaſioned by aberration. There is a retardation ariſing from the aberration of the Sun, as well as from that of Venus. The aberration of the Sun, it is well known, leſſens its longitude about $20''$. and the aberration of Venus, agreeably to your friend's demonſtration, increaſes its longitude at the time of a tranſit $3''.7$. Venus, therefore, and the Sun, at the inſtant of the true beginning of a tranſit, muſt be ſeparated from one another by aberration $23''.7$; and, ſince Venus then moves nearly at the rate of $4'$ in an hour, it will move over 23″.7 in 5′ : 55″. And conſequently, from the inſtant of the *real* beginning of a tranſit, 5′ : 55″ muſt elapſe before it can begin *apparently*.

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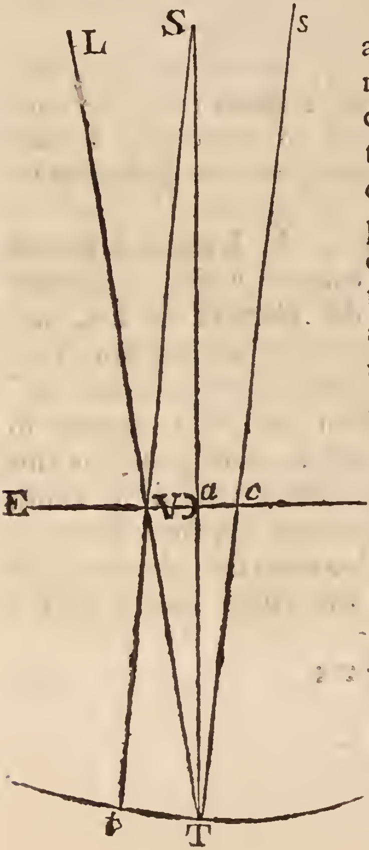
* The paper which occaſioned this letter, and which is here referred to, may be found in p. 358 of this volume.

It may, I know, be objected here, that the aberration of the Sun ought not to be taken into conſideration, becauſe the calculations from the ſolar tables give the apparent places of the Sun, or its longitude with the effect of aberration included, and therefore always about 20″ too little. But from this obſervation a concluſion will follow very different from that which the objection ſuppoſes. The retardation I have mentioned is properly the time that the calculated phaſes of a tranſit of Venus will precede the apparent phaſes, ſuppoſing the tables from which the calculation is made to give the true places of the Sun.

If they give the apparent places of the Sun, this retardation, inſtead of being leſſened, will be conſiderably increaſed. In order to prove this, I muſt deſire it may be remembered, that in deducing by trigonometrical operations the geocentric places of a planet from the heliocentric, the Earth is ſuppoſed to be in that point of the ecliptic which is exactly oppoſite to, or 180° from the place of the ſun, and that this ſuppoſition is juſt only when the ſun's true place is taken. In reality, the Earth is always about 20″ more forward in its orbit than the point oppoſite to the Sun's apparent place; and in conſequence of this it will happen, that in calculating a tranſit of Venus from tables which give the Sun's apparent places, a greater difference will ariſe between the calculated and the obſerved times than if the tables had given the Sun's true places.

For, let S be the Sun, T the Earth, V Venus. Were there no aberration of light, the Sun would be always ſeen in its true place, or in the direction TS. But, in reality, in conſequence of aberration, it will be ſeen 20″ leſs advanced in the ecliptic, or in the direction T*s*, ſuppoſing ST*s* to be an angle of 20″. Now a calculation from tables giving the true places of the Sun, would fix the moment of a conjunction, to the time that Venus gets to TS; but this, though the time of the true conjunction, would not be the time of the obſerved conjunction; for the Sun being then really ſeen in the direction Ts, Venus, after getting to TS, muſt move 20″, or from *a* to *c*, before the apparent conjunction can take place.



But if the calculations are made from the apparent places of the Sun, the conjunction will be fixt to the time Venus gets to *t* S, or a line drawn through S parallel to *s* T, for in this caſe t will be the point of the ecliptic oppoſite to the apparent place of the Sun, and the longitude of the ſun ſeen from *t* will be 20″ leſs than its true longitude, and therefore the ſame with its apparent longitude. But the Earth being then really at T, Venus will, at the calculated time of a conjunction, be obſerved at a diſtance from the Sun equal to the angle L T *s*. This angle, ſuppoſing V T 277, and V S 723, may-be eaſily found to be $72''.2$. Add to this $3''.7$, the proper aberration of Venus at the time of a tranſit, removing it more towards E, and the whole viſible diſtance of Venus from the Sun's center at the calculated moment of a conjunction, will be $75''.9$, over which it will move in 19 minutes of time. And this, conſequently, will be the retardation of the phaſes of a tranſit of Venus occaſioned by aberration, on the ſuppoſition, that in calculating, the Sun's apparent, and not his true place is taken.

I believe theſe obſervations have not been attended to by aſtronomers; and therefore I am the more deſirous of communicating them to you.

I am,

Dear Sir,

with much reſpect,

your obliged humble ſervant,

Richard Price.

P. S. In a former letter which I ſent you, I gave, by miſtake, the error occaſioned by aberration leſs than I have now given it. The diſcovery of this miſtake I owe to the kind aſſiſtance and correction with which Mr. Maſkelyne, the aſtronomer royal, has been pleaſed to favor me.

I have, for the ſake of more diſtinctneſs and clearneſs, ſuppoſed Venus to move in the plane of the ecliptic. Some differences will ariſe from the inclination of the path of Venus to the ecliptic, and alſo from taking the aberration of the Sun, and the proportion of Venus's diſtance from the Earth to her diſtance from the Sun, exactly as they really are at the time of a tranſit. Thus, at the time of the laſt tranſit of Venus, ſuppoſing light to come from the Sun to the Earth in 8′,2, the aberration of the Sun was 19″.8. The diſtance of Venus from the Earth was to its diſtance from the Sun as 290 to 726, and therefore the retardation 18′: 16″.

Mr. Canton has obſerved, that in the *Con. des Temp.* Mr. De la Lande makes the effect of aberration at the inferior conjunction of Venus and Mercury to be an augmentation of their longitudes. Indeed, Mr. Bliſs himſelf obſerves this; and yet, through an overſight, makes the effect as to time to be an acceleration. Vid. Phil. Tranſ. vol. LII. p. 249.

XLVIII. *A Catalogue of the Fifty Plants from* Chelſea *Garden, preſented to the* Royal Society *by the worſhipful Company of Apothecaries, for the Year* 1769, *purſuant to the Direction of Sir* Hans Sloane, *Bart. Med. Reg. et Soc. Reg. nuper Præſes. By* William Hudſon, *Societatis Regiæ et clariſſ. Societatis Pharmaceut.* Lond. *Soc. Hort.* Chelſean. *Præfectus et Prælector Botanicus.*

Read Dec. 6, 1770.

2351 ACALYPHA, *Virginica*, involucris femineis cordatis inciſis, foliis ovato-lanceolatis petiolo longioribus. Lin. Spec. plant. 1423.

Mercurialis tricoccos, hermaphroditica. Pluk. phyt. tab. 99. fig. 4.

2352 Agroſtis, *canina*, calycibus coloratis, petalorum ariſta dorſali recurvatis, culmis proſtratis ſubramoſis. Lin. Spec. plant. 92.

Gramen ſupinum caninum paniculatum, folio varians. Bauh. pin. 1.

2353 Agroſtis, *capillaris*, panicula capillari patente, calycibus ſubulatis æqualibus hiſpidiuſculis coloratis, floſculis muticis. Lin. Sp. pl. 93.

Gramen montanum, panicula ſpadicea delicatiore. Bauh. pin. 3.

2354 Agroſtis, *ſtolonifera*, panicula ramulis divaricatis muticis, culmo ramoſo repente, calicibus æqualibus. Lin. Spec. plant. 93.

Gramen caninum ſupinum minus. Vaill. Paris. 86.

2355 Agroſtis, *paluſtris*, panicula coarctata mutica, calycibus æqualibus hiſpidiuſculis coloratis; culmo repente. Hudſ. fl. Angl. 27.

Gramen miliaceum majus, panicula ſpadicea. Pet. conc. Gram. 118. — Raii ſyn. 464.

2356 Agroſtis, *præcox*, foliis ſetaceis: vaginis angulatis, floribus paniculato-ſpicatis, floſculis baſi ariſtatis. Lin. Spec. plant. 97.

Gramen parvum præcox, panicula laxa caneſcente. Raii ſyn. p. 407. tab. 22. fig. 2.

2357 Amaranthus, *Græcianus*, glomerulis triandris axillaribus, foliis lanceolatis obtuſis repandis. Lin. Spec. plant. 1405.

2358 Amaryllis, *lutea*, ſpatha uniflora, corolla æquali, ſtaminibus declinatis. Lin. Spec. plant. 420.

Colchicum luteum 1. majus. Bauh. pin. 69.

2359 Aſter, *tenellus*, foliis ſubfiliformibus ſcabris, pedunculis nudis, calycibus hemiſphæricis glabris. Lin. Spec. plant. 1225.

2360 Centaurea, *Amberboi*, calycibus inermibus ſubrotundis glabris: ſquamis ovatis, foliis lyratodentatis. Lin. Spec. plant. 1286.

Cyanus orientalis major, foliis magis diſſectis, flore luteo. Moris Hiſt. III. pag. 135. ſec. 7. tab. 25. fig. 9.

2361 Chenopodium, *ariſtatum*, foliis lanceolatis ſubcarnoſis, integerrimis, corymbis dichotomis ariſtatis axillaribus. Lin. Spec. plant. 321.

2362 Coronilla *Cretica*, herbacea, leguminibus quinis erectis teretibus articulatis, foliolis undenis. Lin. Spec. plant. 1048.

Coronilla Cretica herbacea, flore parvo purpuraſcente. Tourn. Cor. 44.

2363 Cucubalus *Tartaricus*, petalis bipartitis, floribus ſecundis decumbentibus, pedunculis oppoſitis ſolitariis erectis caule ſimpliciſſimo. Lin. Spec. plant. 592.

2364 Cynogloſſum *linifolium*, foliis lineari-lanceolatis grabris. Lin. Spec. plant. 193.

Cynogloſſum minus album, linifoliis glaucis, ſemine umbilicato. Moris. Hiſt. III. p. 449. ſec. 11. tab. 30. fig. 11.

2365 Eryſimum *cheiranthoides*, foliis lanceolatis integerrimis. Lin. Spec. plant. 923.

Myagrum ſiliqua longa. Bauh. pin. 109.

2366 Euphorbia *Hiſpanica*, umbella quinquifida: dichotoma, involucellis cordatis, foliis oblongis integerrimis emarginatis, capſulis verucoſis.

Tithymalus latifolius, Hiſpanicus. Bauh. pin. 291.

2367 Feſtuca *myuros*, panicula ſpicata nutante, calycibus minutiſſimis muticis, floribus ſcabris longius ariſtatis. Lin. Spec. plant. 109.

Gramen murorum, ſpica longiſſima. Raii Syn. 415.

2368 Feſtuca *duriuſcula*, panicula ſecunda oblonga, ſpiculis ſexfloris oblongis, lævibus, foliis ſetaceis. Lin. Spec. plant. 108.

Gramen pratenſe, panicula duriore laxa unam partem ſpectante. Raii Syn. 413. tab. 19. fig. 1.

2369 Gypſophilla *rigida*, foliis lineari-lanceolatis planis caule dichotomo, pedunculis bifloris, petalis emarginatis. Lin. Spec. plant. 583.

2370 Hedyſarum *caput galli*, foliis pinnatis, leguminibus monoſpermis criſtatis, aculeatis, petalis æqualibus caule diffuſo. Lin. Spec. plant. 1059.

Onobrychis, fructu echinato minor. Bauh. pin. 350.

2371 Hordeum *Zeocriton*, floſculis lateralibus maſculis muticis, ſeminibus angularibus patentibus corticatis. Lin. Spec. plant. 125.

Zeocriton S. Oriza Germanica. Bauh. pin. 22.

2372 Iris *tuberoſa*, corollis imberbibus, foliis tetragonis. Lin. Spec. plant. 58.

Iris tuberoſa, folio anguloſo. Bauh. pin. 40.

2373 Leonurus *marubiaſtrum*, foliis ovatis lanceolatiſque ſerratis calycibus ſeſſilibus ſpinoſis. Lin. Spec. plant. 817.

Marubiaſtrum foliis cardiacæ. Bocc. Muſ. 2. pag. 120. tab. 98.

2374 Lepidium, *Iberis*, floribus diandris tetrapetalis, foliis inferioribus lanceolatis ſerratis; ſuperioribus linearibus integerrimis. Lin. Spec. plant. 900.

Iberis latiore folio. Bauh. pin. 97.

2375 Lobellia *inflata*, caule erecto, foliis ovatis ſubſerratis pedunculo longioribus, capſulis inflatis. Lin. Spec. plant. 1320.

2376 Lotus *ornithopoides*, leguminibus ſubternatis arcuatis compreſſis, caulibus diffuſis. Lin. Spec. plant. 1091.

Lotus ſiliquis ornithopodii. Bauh. pin. 332.

2377 Melia, *Azedarach*, foliis bipinnatis. Lin. Spec plant. 550.

Arbor fraxini folio, flore cæruleo. Bauh. pin. 415.

2378 Olea *Europæa*, foliis lanceolatis. Lin. Spec. plant.

Olea ſativa. Bauh. pin. 472.

2379 Ononis *ornithopoides*, pedunculis bifloris ariſtatis, leguminibus linearibus cernuis. Lin. Spec. plant. 1009.

Fœnum Græcum ſiculum, ſiliquis ornithopodii. Tourn. 409.

2380 Origanum, *Majorana*, foliis ovalibus obtuſis, ſpicis ſubrotundis compactis pubeſcentibus. Lin. Spec. plant. 825.

Majorana vulgaris. Bauh. pin. 224.

2381 Panicum *capillare*, panicula capillari erecta patente, caule hirto. Lin. Spec. plant. 87.

Panicum miliaceum viride, foliis latis brevibus, panicula capillacea. Sloan. Jam. 1. pag. 115. tab. 72. fig. 2.

2382 Paſſiflora *cærulea*, foliis palmatis integerrimis. Lin. Spec. plant. 1360.

Flos paſſionis major pentaphyllus. Sloan. Jam. 1. pag. 229.

2383 Phleum *paniculatum*, panicula cylindrica ſpiciformi, glumis glabris, culmo ſubramoſo. Hudſ. Fl. Angl. 23.

2384 Poa *annua*, panicula diffuſa angulis rectis, ſpiculis obtuſis, culmo obliquo compreſſo. Lin. Spec. plant. 99.

Gramen pratenſe paniculatum minus. Bauh. pin. 2.

2385 Poa *trivialis*, panicula ſubdiffuſa, ſpiculis trifloris baſi pubeſcentibus, culmo erecto tereti. Lin. Spec. plant. 99.

Gramen pratenſe paniculatum medium. Bauh. pin. 2.

2386 Poa *pratenſis*, panicula diffuſa, ſpiculis quinquefloris glabris, culmo erecto tereti. Lin. Spec plant. 99.

Gramen pratenſe paniculatum majus latiore folio. Bauh. pin. 2.

2387 Poa *anguſtifolia*, panicula diffuſa, ſpiculis quadrifloris pubeſcentibus, culmo erecto tereti. Lin. Spec. plant. 99.

Gramen pratenſe paniculatum majus anguſtiore folio. Bauh. pin. 2.

2388 Poa *loliacea*, ſpicata, ſpiculis alternis ſeſſilibus, ſubſexfloris. Hudſ. Fl. Angl. 35.

Gramen pumilum loliaceo ſimile. Raii Syn. 395.

2389 Polycnemum *Hiſpanicum*. Lin. Spec. plant. 50.

Camphorata congener. Bauh. pin. 486.

Camphorata vaginis ſpinoſis. Hall. Helv. 183.

2390 Polypodium *aculeatum*, frondibus bipinnatis: pinnis lunulatis ciliato-dentatis, ſtipite ſtrigoſo. Lin. Spec. plant. 1552.

Filix mas aculeata major. Bauh. pin. 358.

2391 Polypolium *lobatum*, fronde bipinnata pinnis lanceolato-ovatis dento-ciliatis ſuperne baſi lobatis ſtipite ſtrigoſo. Hudſ. Fl. Angl. 390.
Filix lonchitidis affinis. Raii Syn. 121. Pluk. phyt. tab. 180. fig. 3.

2392 Rhus *radicans*, foliis ternatis: foliolis petiolatis ovatis nudis integerrimis, caule radicante. Lin. Spec. plant. 381.

2393 Ricotia *Ægyptiaca*. Lin. Spec. plant. 912.
Lunaria foliis ſupradecompoſitis: foliolis trifidis, ſiliquis oblongis pendulis. Mill. Icon. 169.

2394 Salvia *hæmatodes*, foliis cordato-ovatis rugoſis tomentoſis, calycibus hiſpidis, radice tuberoſo. Lin. Spec. plant. 35.
Horminum ſylveſtre majus hæmatodes glabrum, flore cœruleo, Italicum. Barr. tab. 185.

2395 Salvia, *Horminum*, foliis obtuſis crenatis, bracteis ſummis majoribus coloratis. Lin. Spec. plant. 34.
Horminum ſativum. Bauh. pin. 34.

2396 Seriola *æthnenſis*, hiſpida foliis obovatis ſubdentatis. Lin. Spec. plant. 1139.
Hieracium multicaule, bellidis folio molli. Bocc. app. 8.

2397 Seſeli *annuum*, petiolis rameis membranaceis ventricoſis emarginatis. Lin. Spec. plant. 373.
Fœniculum ſylveſtre annuum, tragoſelini odore, umbella alba. Vaill. Pariſ. 54. tab. 9. fig. 4.

2398 Sinapis *lævigata*, ſiliquis lævibus patulis, foliis lyratis glabris, ſummis lanceolatis caule lævi. Lin. Spec. plant. 934.

2399 Sinapis *erucoides*, ſiliquis lævibus, foliis lyratis oblongis glabris, caule ſcabro. Lin. Spec. plant. 935.

Eruca ſylveſtris, flore albo, Italica. Barr. Icon. 132.

2400 Teucrium *chamæpitys*, foliis trifidis linearibus integerrimis, floribus ſeſſilibus lateralibus, caule diffuſo. Lin. Spec. plant. 787.

Chamæpitys lutea vulgaris, ſ. folio trifido. Bauh. pin. 249.

XLIX. *A ſhort Account of the Obſervations of the late Tranſit of* Venus, *made in* California, *by Order of his Catholic Majeſty; communicated by his Excellency Prince* Maſſerano, *Ambaſſador from the* Spaniſh *Court, and F. R. S.*

Read Nov. 22, 1770.

DON Vincent Doz, Commander of a Spaniſh frigate, is juſt arrived at Madrid. He brought with him, and preſented to the King, an account of his obſervation of the laſt Tranſit of Venus at California, whither he was ſent laſt year for that purpoſe, being in ſubſtance as follows:

	d	'	''
The latitude of the Village of St. Joſeph, eight leagues diſtant from Cape St. Lucar - - - - - - - - -	23	5	15
	h	'	''
The longitude from the meridian of Paris - - - - - - - - - - -	7	28	17½
The two internal contacts of the Planets were at	0	17	25
and at	5	54	44½
Hence, on the computation of Monſ. Pingré, in his Memoir of the year 1767, the ſolar Parallax is - - -	0	0	8¼

And the diſtance of the Sun from the Earth is greater than it was ſuppoſed to be $\frac{7}{11}$, or nearly 6,685,000 leagues.

The correſponding obſervations will be neceſſary to reſolve the problem more compleatly.

II. Extract

L. *Extract of a Letter, dated* Paris, Dec. 17, 1770, *to Mr.* Magalhaens, *from M.* Bourriot; *containing a ſhort Account of the late Abbé* Chappe's *Obſervation of the Tranſit of* Venus, *in* California. *Tranſlated by Dr.* Bevis, *F. R. S.*

. . . . THE 7th of December inſtant, the Journals and Manuſcripts of the late Abbé Chappe were depoſited at the Royal Obſervatory, with M. Caſſini de Thury, by the Sieur Pauli, one of the King's Engineers, and Geographers, who had accompanied the Abbé in his voyage to California. M. Pauli relates, that M. Chappe choſe to ſtay at St. Joſeph, a ſmall village ten leagues from St. Lucar, notwithſtanding the contagious diſeaſe prevailing there, and relying on his own good conſtitution; becauſe he had no more than eight days to prepare for his Obſervation.

Eight days after the Tranſit, he ſickened, yet continued his obſervations to the 18th of July; and, a little before his death, left his materials in writing, put into a box, with M. Pauli, to be delivered to the Royal Academy. He died about the 1ſt of Auguſt, as did, about the ſame time, the Clock-maker, the Interpreter, one of the two Spaniſh Officers, beſides 12 Soldiers, and four Officers ſent from Mexico, and about 50 Indians.

	h	m	s
The firſt internal Contact was at - -	0	17	27
The ſecond Contact at - - - - -	5	54	$50\frac{3}{10}$
The Duration - - - - - - - - -	5	37	$23\frac{2}{10}$
	°	′	″
The Latitude of the Place - - - -	23	3	37
Laſtly (according to M. de la Lande) the Parallax of Venus - - - -	0	0	$8\frac{1}{2}$

and her diſtance about 35000000 leagues of 2283 toiſes each, upon a mean of compariſons with obſervations made in the North of Europe, at Cajaneburg and Wardhus.

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OF THE
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257,	2,	XXIII.	XXIII. *
258,	column 6th, l. 13,	30	80.
259,	25,	30	80.
261,	16,	refractor	reflector.
262,	22,	would	could.
264,	2,	refractor	reflector.
268,		XXIV.	XXIV. *
289,	13,	haurendæ	hauriendæ.
417,		XXXIV.	XXXIV. *
456,	near the bottom,	Sipca,	Spica.

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